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EDITED BY J. McKEEN CATTELL

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THE SCIENTIFIC MONTHLY

JULY, 1927

A PARADISE FOR PLANT LOVERS

THE BIOLOGICAL STATION AT ALTO DA SERRA, BRAZIL

By Dr. ALBERT F. BLAKESLEE

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ONE's early idea of tropical vegetation is generally obtained from books. It is often with some disappointment, therefore, that in his travels near the equator the tourist fails to find the luxuriant growth of plants of which he has read. The reason the ordinary traveller does not see the optimum vegetation is largely due to the fact that the tourist and business routes in the main avoid localities where the precipitation of water is abundant throughout the year. Where the temperatures are favorable, as is the case in the lower altitudes in the tropics, the most common limiting factor for luxuriant plant growth is the supply of water.

The influence of moisture on the type of vegetation is well shown in islands in the Caribbean where mountains cause precipitation from the moist trade winds and leave the leeward areas on the south relatively dry. Thus, Kingston in the south of Jamaica has a rainfall of around twelve inches and shows a dry-climate vegetation of cacti and other tropical xerophytic plants, while on the north and less populated side of the Blue Mountains a precipitation of as many feet has been reported and the vegetation is accordingly luxuriant. In the Hope Botanic Gardens, in Kingston, Jamaica, an interesting collection of tropical plants is assembled, but the need of more moisture is shown by the presence of a water faucet at the base of each palm tree.

Botanical gardens in South America, as elsewhere, are outdoor museums of plants with an orderly arrangement of living specimens and tend to emphasize the exotic or the rarer elements in the native flora. The Biological Station at Alto da Serra in the neighborhood of São Paulo, Brazil, seems unique in that it not only presents a well-nigh unparalleled luxuriance of virgin plant growth brought about by the humid semi-tropical climate, but, in addition, is located on a main line of travel. In many places it would be almost impossible to penetrate the dense tangle of growth, but a series of foot paths have been cut through, making the interior accessible, but otherwise the place has not been "improved" by the hand of man. It is, in fact, the ideal of Professor F. C. Hoehne, the director of botanical activities in São Paulo, to keep this biological station a virgin sanctuary for native plants and animals—not a cleared park or botanic garden of rare specimens. Trees fall and moulder where they lie. Epiphytes of various species carpet them with green patterns of different shades,

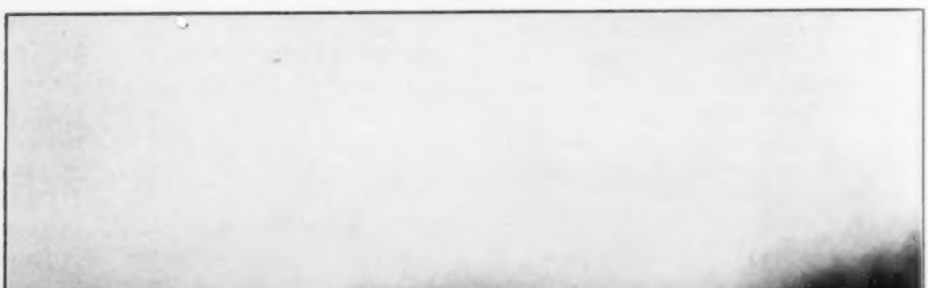
VIEW LOOKING TOWARD SANTOS

FROM ENTRANCE TO BIOLOGICAL STATION AT ALTO DA SERRA, BRAZIL. EXPOSURE WAS TAKEN IN EARLY AFTERNOON. PHOTO BY A. F. BLAKESLEE.

leaving at length ridges of varied verdure on the forest floor.

It is a matter of surprise that such a paradise for plant lovers is so little known, even to professional botanists. It had not been mentioned by any one from whom we sought advice regarding our four months' botanical trip around South America as a place that ought to be visited until we were at São Paulo, when Professor Hoehne described the station and suggested our visiting it the next morning. Our plans had been made, however, to leave for Rio on the morning train and we inquired about the possibility of going in the afternoon, but he replied, "You had better stay over another day and go in the morning because it will rain this afternoon. It always does rain at Alto da Serra in the afternoon, but the mornings are clear." Since it was not convenient to change our schedule, he offered to join us in gambling on the afternoon weather and to make the trip after lunch.

Alto da Serra is a railroad station



THE SAME VIEW AS THAT ABOVE

BUT EXPOSURE WAS TAKEN IN THE LATE AFTERNOON. IT SHOWS THE CLOUDS ROLLING UP OVER THE LANDSCAPE—TYPICAL OF AFTERNOONS AT ALTO DA SERRA. PHOTO BY A. F. BLAKESLEE.

with a scattering of houses at the summit of the scenic railroad climb from the port of Santos on the Atlantic to São Paulo, and about an hour's run from the latter city. It has an elevation of about 2,400 feet, and the ridge on which it is located intercepts the moist breezes from the nearby ocean and causes an almost daily precipitation of water in the form of rain, mist or fog. The official records for the little village give an annual rainfall of 145 inches, with 161 days in the year in which rain was recorded. Judging from our experience, it is probable that on many of the days on which no precipitation was recorded in the rain gauge, the clouds must have hung over the ridge in the afternoon and from time to time enveloped the vegetation. We were fortunate in our afternoon since it actually rained but little, but the fog, when no rain was falling, collected on the foliage as well as on our garments. The type of vegetation showed that the air was almost continuously laden with moisture. On our earlier trip up from Santos we had remarked on the fact that at Alto da Serra, where we changed engines, was the only group of wooden houses we had seen in South America. Professor Hoehne told us they were made of wood in order to keep the dwellings drier in the moist climate. It is the constant moisture combined with the subtropical temperatures here—it is only a few miles south of the tropic of Capricorn—that affords optimum conditions for plant growth.

A short stone's throw from the railroad station at Alto da Serra, a winding path leads up into the biological station. Fuchsias, begonias and other plants familiar to us only in greenhouses bordered the pathway. Tree ferns were common as well as delicate filmy ferns (Hymenophyllaceae), which grow only where moisture is abundant and continuous. We stopped a moment for a view of the landscape looking down toward Santos

A WINDING FOOT PATH IN THE BIOLOGICAL STATION

SHOWING A YOUNG TREE FERN AT RIGHT AND A MOSS-COVERED BANK AT LEFT. PHOTO BY A. F. BLAKESLEE.

and the ocean. Later in the afternoon, on our return, we saw from the same spot the clouds rolling up the slopes like smoke. Paths named after biologists who had visited the place intersect the reservation in various directions. These footpaths have been well planned to give access to the different types of vegetation within the station, and their excellent condition and easy grade were appreciated by the women of our party.

The station is six kilometers long by one and a half wide and includes in its nine hundred acres a wide diversity in

MAXILLARIA PICTA
A VERY INTERESTING EPIPHYTIC ORCHID FOUND IN THE BIOLOGICAL STATION. THE FLOWERS, SEEN TOWARD THE BASE OF THE NARROW LEAVES, ARE WHITE THICKLY SPOTTED WITH PURPLE. PHOTO BY F. C. HOEHNE.

soil and altitude. In consequence, the flora is extremely varied. Open meadows as well as virgin forests are encountered within its limits. A wooden building affords a dwelling for the caretaker as well as a rest room and laboratory for visitors.

In his beautifully illustrated volume recently published on the Botanical Department of the São Paulo museum, Professor Hoehne has given some statistics regarding the species of plants which will afford an idea of the richness of the flora. Within the confines of the biological station there have been identified so far about six hundred species of trees and shrubs; thirty to forty species of Bromeliads; more than two hundred species of mosses and hepatics; about one hundred and fifty orchids; about fifty each of Melastomaceas and *Rubiaceas* and as many species of ferns; about a dozen different palms and a dozen species of Begonias; and some hundreds of species representing other less common families. Not only is the ground covered with vegetation, but the trunks and branches of the trees as well. A tree was pointed out to us on which twenty different species of orchids has been found growing, as well as an abundance of other epiphytes.

The orchids were of especial interest to one who was not familiar with them outside of conservatories. One species is so small that the whole plant could be included in a circle one centimeter in diameter—half the size of an American nickel. We saw growing on a trunk one moss-like orchid that was twice this size, but it had such minute flowers that we were obliged to use our pocket magnifying glass to make out its perfectly formed floral structures and to satisfy ourselves that it was in fact an orchid.

We can confirm the testimony of the much-travelled Belgian botanist, Professor Jean Massart, who writes that he knows of no station in the world so interesting as Alto da Serra. He adds that the reserves belonging to the famous Botanical Gardens of Buitenzorg in Java, which botanists have generally considered to have the most luxuriant tropical vegetation of the world, has a less varied flora.

It is not appropriate here to give a detailed description of this intensely interesting biological station. I wish merely to call its attention to travellers in South America who are interested in plants. It is my suggestion that, when they are at the famous snake farm at Butantan in São Paulo, they visit the nearby botanical building and make arrangements with Professor Hoehne or with some one of his associates for a trip in the morning to the biological station at Alto da Serra.

A VIEW IN THE VIRGIN FOREST

Showing the common "Jussara" palm (Euterpe edulis) sometimes called the Assai palm from the fact that its seeds are used in the production of a drink known as assai. The long pendant roots of Philodendron eximium, more commonly called "Imbe" are seen hanging down from the leafy canopy at the right of the picture. Photo by F. C. Hoehne.

A BEAUTIFUL GROUP OF JUSSARA PALMS
IN THE PLACE WHICH HAS BEEN CALLED "PARK OF THE JUSSARAS." PHOTO BY F. C. HOEHNE.

A GROUP OF GEONOMA PALMS (G. SCHOTTIANA)

This graceful species called "Guaricanga" or "Ubim" in Portuguese, is an undergrowth form and not like the tall palms more familiar in open places. Photo by F. C. Hoehne.

A VIEW LOOKING UP THE FOOT PATH "WASHINGTON LUIS"
NAMED AFTER THE PRESENT PRESIDENT OF BRAZIL. THE FOG IS ABOUT TO ENCIRCLE THE FOREST.
PHOTO BY F. C. HOEHNE.

VIEW ALONG THE FOOT PATH "DR. FREDERICO VON MARTIUS"
THE FOG CAN BE SEEN ROLLING IN TO MEET THE OBSERVER. PHOTO BY F. C. HOEHNE.

TREES ARE SHOWN OVERGROWN BY ORCHIDS AND BROMELIADS
THE SURFACE OF A TRUNK IS OFTEN COMPLETELY COVERED FROM BOTTOM TO TOP.
PHOTO BY F. C. HOEHNE.

AN OLD TREE
WHICH IS ACTING AS HOST TO MANY INTERESTING AND BEAUTIFUL ORCHIDS. PHOTO BY F. C. HOEHNE.

INTERIOR OF THE VIRGIN FOREST OF THE BIOLOGICAL STATION
PHOTO BY F. C. HOEHNE.

AN ASCENT WITH STEPS IN THE PATHWAY "WASHINGTON LUIS"

ALONG THIS PATHWAY ONE MAY CLIMB FROM A HEIGHT OF 600 METERS (ABOUT 2,000 FT.) ABOVE SEA LEVEL TO AN ELEVATION OF 900 METERS (NEARLY 3,000 FT.). PHOTO BY F. C. HOEHNE.

A SCENE FRAMED IN TREES

FROM THE HIGHEST POINT OF THE PATHWAY "DR. ADOLPHO LUTZ." PHOTO BY F. C. HOEHNE.

THE GROWTH OF EXPERIMENTATION IN THE EARLY SCIENCES

By Dr. EDWARD F. ADOLPH

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To-day experimentation is *the* method of science. It was not always so; indeed, it has been so for but a short span in the history of progress. Whence came this method, and what did it supplant? Other methods have in other ages been just as sovereign, and indeed experiment was once anything but orthodox.

At the outset we meet a large difficulty in describing just what we mean by experiment, and then a larger difficulty in describing what our predecessors in science meant by experiment. To some an anatomical dissection is an experiment, in fact, Vesalius is often called the first of the experimenters; to some the observation of those changes which occur as consequences of variations of nature constitutes experiment; to others a measurement obtained by the use of complicated laboratory apparatus is an experiment.

It might satisfy the requirements of science's present status to say that, in physical terms, every system is composed of and environed by a number of invariable factors and a number of variable factors. Any description which takes into account one or more variable factors is an experiment. This would include some of the mathematician's investigations as well as what nature presents to the pathologist.

But to Pliny an *experimentum* was a testing-out process. Thus, a test for the toxicity of the air in a well was to let a candle down into the well and see if its light went out. To the Roman of the decadent period, Hero of Alexandria, for instance, an *experimentum* was a secret of nature, such as the transmutation of metals; or sometimes merely a marvel of nature, as the annual overflow of the Nile. Later, to Philostratus, for example, an *experimentum* was an entertainment stunt, such as a sleight-of-hand performance. To William of Auvergne an *experimentum* was the manipulation of the strange instruments which the court astrologer, and sometimes the court jester, exhibited before the king. Thorndike[2] says that: "Throughout the period from Galen to Roger Bacon experiment meant only one thing, namely, the performance of magic. There was no implication of an attempt to get at causes."

It is necessary, therefore, in order to extract information of scientific significance from early sources, to disregard what Seneca or Pliny *called* an experiment and to discover what experiments, in *our* sense, were actually performed. One must not be disappointed to find that those who performed these experiments were not proud of them; indeed, experiments were regarded as such unimportant products of material manipulation that the records made of them were fragmentary and incidental.

In past ages the scale of values and the intellectual tradition were in sharp contrast to those which have produced the present-day universality of experimentation. The contrast shows, for one thing, how efficient and effective a factor in creative work is the apparently harmless philosophy which forms the background. Animistic theology, which

[2] Lynn Thorndike, "A History of Magic and Experimental Science." New York, 1923, Vol. 1, page 21.

seems to have constituted the earliest mental content of our race, implied that an understanding of nature came by revelation. Crudely put, the existence of immortal entities was the starting point from which the forms of earthly existence could be deduced. These were like the axioms in geometry; granted them, the rest was just reasoning and contemplation. It was dangerous to one's status in the community to ask whence came the axioms. The purposeful experiment, then, would be a doubt of an axiom; it took a heretic to perform it; no wonder he did not herald it as the introduction of a new method of progress.

Nevertheless, experiments were performed and made use of. In evaluating them it is always hard to tell whether one is reading too much or too little between the lines. Thus, the philosopher Pythagoras (582–489) is credited with discovering the numerical relations of auditory pitch. Gomperz believes that in this Pythagoras made the "luckiest hit in the history of science." But in the estimate of Allbutt,[3] "Pythagoras seems to have initiated his experiments for the very reason that subjective standards were variable and fallacious." Boethius is the source of the story that Pythagoras, listening to the clanging of hammers on a smith's anvil, noticed that some combinations of blows made harmonious sounds, others did not. Pythagoras seemed to find, on examining the hammers, that neither their shape nor the force with which they were struck was responsible for the pitch, but only their weights. Upon this hint he went home and stretched strings of a uniform kind to different extents by hanging weights on them. The notes emitted by these strings when struck were said to bear simple relationships to the weights on them. But as a matter of fact, neither the weights of hammers nor the weights or tensions upon strings are proportional to the frequencies of vibration; Boethius' tale would therefore not account for Pythagoras' discovery. The correct numerical relationship would be given by the relative lengths of strings placed under uniform tensions. The stringed musical instruments of Pythagoras' day undoubtedly used this principle; perhaps Pythagoras had the scientific curiosity to measure the strings.

With Socrates and Hippocrates came the first philosophic demonstration that some knowledge, at least, was to be derived by induction from facts, rather than by deduction from axioms. Socrates' (470–399) material was gathered chiefly in the field of ethics, and it is very difficult to find any instance where a decisive experiment (in the modern sense) was performed by him. But in the works of Hippocrates' (460–370) school are recorded several experiments which to the modern ear sound not only prototypical but intentional.

Neuburger[4] describes an instance where Hippocrates made use of nature's experimental material. The father of medicine says that in the case of suicides section of the windpipe produces a loss of voice; from this fact it is deduced that voice is caused by resonance of the air inside the windpipe. A more intricate experimental procedure is given as proof of the competence of the semilunar valves. "If a heart be taken out of the body, and of the two valves, one be supported and the other allowed to hang free from the walls, neither water nor air impinging upon them will be able to effect an entrance into the heart."

In the brief treatise "On the Heart" Hippocrates[5] describes a demonstration

[3] T. C. Allbutt, "Greek Medicine in Rome." London, 1921, page 96.

[4] Max Neuburger, "History of Medicine." Translated by Ernest Playfair. London, 1909, Vol. I, page 152.

[5] Hippocrates, "De corde." Editionem curavit D. C. G. Kühn. Lipsiae, 1825, Vol. XXI, pagina 485.

that, in drinking, some of the fluid passes into the windpipe, and, as he thought, goes to the lungs. "If one administers water colored with ochre or vermilion to an animal almost dying of thirst, preferably a pig, and while it is drinking one cuts its throat, he will find the windpipe colored by the potion." The work "On the Heart" is not considered as genuine among those works credited to Hippocrates. Certainly, however, it was written in the Coan school, at some time before the Roman period.

But Socrates and Hippocrates put no true reliance upon experimentation; it was not recognized as a method of discovery, and their followers almost abandoned it. Plato indeed was interpreted for twenty centuries to hold that experience was of no value. On the other hand, Aristotle (384–322) practiced the art of observation and, at least as far as he was himself concerned, laid the foundations of empirical science. We find almost no record of a real experiment, however, in the whole of the works of Aristotle, unless it be that the opening of hen's eggs on successive days of incubation be an experiment, with time as the variable. It depends on one's latitude in using the designation of experimentation. Thus Allbutt[6] says that "Aristotle made occasional experiments; for instance, on the alleged immunity given by marjoram to tortoises which were thereby enabled to eat snakes with impunity." In this experiment the peripatetic evidently omitted to test control animals. It is interesting to note in this connection that the introduction of the inductive method by Socrates and Aristotle also initiated specialization in intellectual work. For empiricism was a recognition of the failure of Thales, Anaxagoras and others to show the connection of their axioms with common sense.[7]

[6] Allbutt, *op. cit.*, page 129.

[7] W. Whewell, "On the Philosophy of Discovery." London, 1860, page 33.

A certain pupil of Aristotle who was a physician, named Menon, wrote a comprehensive treatise on the history of medicine which is now completely lost. The treatise was used as an authoritative text for some centuries, and certain fragments, supposedly from a student's notebook of 150 A. D., indicate that the treatise contained descriptions of some actual experiments. In particular, the influences of feeding, upon the losses and gains of weight by the body, are mentioned. This is notable as the first recorded attempt at exact measurement in physiology, and an early anticipation of the use of the balance by Nicolaus of Cusa and Sanctorius.

Archimedes (287–212) is sometimes pictured as the prototype of the experimenter. Whether or not we should consider him so depends upon whether we look merely at his results or whether we look at his method of work. For Heath[8] has shown that Archimedes' levers, pulleys and water-screws were used by Archimedes as ocular demonstrations of his deductions, and not as the starting point for inductions. His so-called experiments, those that impressed his contemporaries, are almost without mention in his own work. Thus in his treatise "On Floating Bodies"[9] there is no mention of the determination of the specific gravity of gold in the famous testing of the crown. Archimedes experimented, but not inductively.

This introduces the generalization that every science seems to exhibit a cycle in the methods by which it does its work. At first there is speculation, in the second stage observation and thirdly experimentation. These three processes are inductive. Once generalizations have been reached the science passes into the final or deductive stage. All the stages of induction were very brief in the case of geometry; so brief that even

[8] T. L. Heath, "The Works of Archimedes." Cambridge, 1897, page xiv.

[9] Heath, *op. cit.*, page 258.

Euclid failed to realize that his axioms resulted from this process. Yet in the work of Pythagoras it seems fair to conclude that geometry was in an observational stage. We might even consider as experimental arithmetic the arrangements of spaced dots which the Pythagoreans used.

In other sciences induction has been a much more prolonged process; but many fields of physical science are now largely deductive, while in physiology there is yet hardly anything from which to deduce. In causal morphology and pathology the experimental stage has barely begun, while in anthropology observation has barely succeeded speculation.

So at any one time we may find sciences in all stages of evolution. Also we may judge of the completeness of various sciences by tracing the relative durations and complexities of the various stages in their history. And this leads to the suggestion that here lies a value in the history of science which is only beginning to be recognized: from its study it is possible to piece together the elements of a science of science.

The biological sciences, as well as the physical sciences, relied upon nature's experiments for many centuries. As astronomers like Claudius Ptolemy were able to induce the principles of light and motion from examination of the heavens, so biologists such as Pliny and medical men such as Oribasius were able to generalize and systematize their facts from the comparison and the pure observation of what they *happened* to see. Seneca, diligently digging in his vineyard, found that no rain was ever so heavy as to moisten the earth to a depth of more than ten feet. That might have been an experiment, had he set himself a question before he dug, but actually he drew merely upon his general experience when he concluded that therefore "all the moisture is consumed in this outer crust and descends not to the lower part." He laid no building stone for geology, after all.

There is some indication that experiments were actually performed in the Alexandrian schools of medicine. One need not refer to the oft-quoted test by which Erasistratus (350–280) diagnosed, and the diplomacy by which he secured a cure for, the lovesickness of King Ptolemy's son.

Erasistratus kept some chickens in a jar, weighing them and their excreta at intervals. He weighed them after feeding, and after digestion was completed. During starvation he found that more weight was lost by the birds than was contained in the excreta. This he said was due to insensible excretion. It is not clear whether this experiment was related in idea or in act to the experiment accredited to Menon.

Erasistratus seems to have understood the valves of the heart, and to have correctly stated their action. If not experimentation, this bit of physiology at least involved an amount of intentional observation worthy of original investigation. It often seems as though that which is an experiment in one age is merely a controlled observation in the next; there is a factor of boldness which is present only the first time. Perhaps to Herophilus the greatest experiment was when he cut into his first human cadaver.

In the works of Pliny (23–79), that omnivorous scavenger of fact and fiction, we find several instances of well-recognizable experiments. One that is remarkably modern is the marking by an ancient "scientist" of a dolphin's tail, in order if possible to determine its length of life; and in fact Pliny says that this experiment was completed some three hundred years after its inception by the finding of the dolphin. Another is the sinking of a well, to prove by its complete illumination that the sun casts no shadow at noon of the summer solstice. This procedure strangely reminds one of the troublesome experimental method by which roast pig is said to have been devised. A still less

modern experiment is the casting of a man into a pit of serpents at Rome to see if he was immune to their stings. Pliny also credits the physicians of his day with a great aptitude for experiments, for he says in quite modern vein that the doctors learn at our risk and gain experience through our deaths.

It is safe to say that Greek and Roman science was successful wherever the deductive method was sufficient. An early philosophy of inductive science is contained in the works of the astronomer Claudius Ptolemy. He says that to avoid errors one should repeat accurate observations at long intervals. Further, from the observations one must construct the simplest hypothesis consistent with the facts. This marks a new epoch of objectivity when compared with the stolid deductive universe of Anaxagoras or with the later use of experiment to illustrate the value of reasoning by Archimedes.

We now come to Galen (130–200). In his works are clear descriptions of several first-class experiments; and we are certain that the procedures described were actually carried out by him instead of being merely projected, as in the case of many earlier authors, and that they were original with him. One of his most significant experiments was the refutation of Erasistratus' teaching that the arteries contained air. Galen bound at both ends the femoral artery of a freshly killed animal, and showed that its lumen was filled with blood and had no room for air.

Galen was not the first biologist to be called upon to give public demonstrations of experimental phenomena. But it was under such encouraging circumstances that he showed the connection between the brain, the voice and the breathing. Galen while still a student had become interested, in the sense of a modern investigator, in the movements of the chest in breathing. He it was who first realized that the diaphragm and intercostal muscles were alone responsible for the movements of the lungs. He discovered that by cutting various nerves and various parts of the spinal cord he could tell which nerves initiated the movements involved.[10] This required a great many trials and led him to systematically cut all the nerves of the chest. While doing so he happened upon a definite response to the cutting of the recurrent laryngeal nerve. Walsh[11] has reconstructed for us just how the experiment must have proceeded. "The pig was tied on the table without an anesthetic, and only kept intermittently from squealing by the hardy hands of a slave. The recurrent laryngeal was cut, and in a dramatic fashion the squealing stopped instantly."

It appears that during a number of years Galen carried on intermittently his truly great experiments investigating the distribution of nerves from the brain and spinal cord. He sectioned and hemisectioned the cord at various levels. In this way he demonstrated that the chief nerves arose in the brain. At one stroke Galen devised the method for the investigation of the central nervous system by which has been derived all our detailed knowledge of localization and paths; and single-handed he advanced neurophysiology to the place where it stood until the work of Magendie and Bell about 1810.

The presence of real experimental technique in Galen's work is illustrated by his improvement of Erasistratus' diagnosis of love-sickness. A girl, who showed no dangerous symptoms, was found to quicken her pulse when some one came in from the theater and said that he had just seen Pylades dance. Next day Galen arranged a control by

[10] Claudius Galenus, "De anatomicis administrationibus." Editionem curavit D. C. G. Kühn. Lipsiae, 1821, Vol. II, paginae 675, 842.

[11] Joseph Walsh, "Galen's Discovery and Promulgation of the Function of the Recurrent Laryngeal Nerve." *Ann. Med. Hist.*, Vol. VIII, 176–184, 1926.

having some one come in and describe how Morphus had danced; the pulse did not change. On the third day Pylades' name was again mentioned, and again the pulse quickened. One gains an impression of Galen as a great poser; one who found his science an undeveloped means of making a startling diagnosis or an awe-inspiring demonstration.

Among the isolated instances of experiment previous to Galen, which have been enumerated, it is evident that most were purely incidental. But, at least, Galen and his predecessors had what Pasteur labelled the prepared mind. Alchemy presents a long history of hit-or-miss "experiments" from which almost nothing of scientific value was derived. Of deliberate experiments for scientific purposes, the best examples are those of Archimedes; but he merely did the experiments for the sake of showing the correctness of his deductions. The deliberate experiment which really bore scientific fruit was probably quite without precedent in any field of knowledge when Galen sectioned the spinal cord. Thus through several centuries we might trace the evolution of definiteness in experimentation.

It may be some comfort to those who can still remember the first heralded successes of exact experimental methods in some of the biological sciences, such as zoology and botany, to realize that biology really commenced to experiment before any other science.

But the subsequent fate of biological experimentation has not been so picturesque. One finds discussions of experimental method in the compilations and commentaries of Galen's successors from Paul of Aegina to Adelard of Bath. Experimental method was something to be pigeon-holed and labelled, like the syllogism in logic. Roger Bacon relegated experimental science to a separate field of knowledge; what we should call applied science. To him, as to Archimedes, experiment was the result of deduction. This is evident in the passages where he put forward his fine projects for microscopes and artificial rainbows, which have been interpreted to represent his actual accomplishments.

Nevertheless, as Allbutt[12] has said, "Roger Bacon proclaimed, what even Aristotle scarcely comprehended, that casual experiments are but curious incidents, not indeed in divination of remote or primary causes wherein effects are to be found, nor again in searching behind phenomena for their essences or formative substances, but in detecting and concatenating by the experimental method the series in which they occur."

Meantime, experiments of a systematic character were carried out, very probably, in the field of optics. These start with the studies of refraction by Cleomedes, and the analysis of reflection and visibility by Ptolemy. This was the one field to which the Arabs seem to have contributed true experiments, represented in the writings of Albategnius, Alhazen and Algazel.

With the return of learning to Europe the records of experiments increased in number. Gradually the isolated experiments of the awakening age come to have a meaning; and one can trace their gradual evolution, however incidental their character, in the work of Albert of Cologne, the Holy Roman Emperor Frederick II, Nicolaus of Cusa, and others. Despite the brilliant attempts at experiment by Leonardo da Vinci, Andreas Vesalius, Bernard Palissy and Bernardino Telesio, it remained for Galileo Galilei and William Harvey, only three hundred years ago, to combine the practice of experimentation with the recognition of experiment as a sovereign method of systematically answering questions.

12 Allbutt, *op. cit.*, page 501.

THE SOLID GROUND OF NATURE[1]

By Dr. PAUL R. HEYL
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An ancient Greek legend tells of three brothers who visited the oracle at Delphi. Why they went there we are not told. Possibly they went into the temple "for fun," as a party of young people to-day might stop in at a gypsy camp on passing to have their fortunes told. But there they were, three handsome youths, standing in awed silence before the Pythoness, that mystic priestess, waiting for her to speak.

The priestess chewed the leaves of the sacred laurel and drank of the water of the underground stream; then seating herself upon the tripod she gazed steadily at the youths and said: "To one of you shall be given riches and honor, power and glory."

The young men looked at each other, the unspoken question in their eyes: "Which?"

The Pythoness bent forward on her tripod and inhaled the mephitic vapor that arose from a fissure in the earth. The trance was rapidly coming on her and her utterance became fainter, but before losing consciousness she muttered: "To him who after his return home shall first kiss his mother."

The young men were very silent on their homeward way. As if by tacit understanding the two older ones fell a little behind their younger brother. Between the older youths there was scarcely a year's difference, and no perceptible physical advantage in either. They eyed each other furtively for a time, and then the older spoke.

"This should lie between us." The other nodded slightly.

"*He* is not to be considered." Again a silent nod.

"Shall we leave it to Dame Fortune? Shall we draw lots?" A third nod, and the two walked in silence for some time. In the mind of each was the same thought—that if fortune failed him he would make a last desperate appeal to force.

The youngest brother walked on, deep in thought. Suddenly he raised his head and quickened his pace. Their home was in sight. His brothers behind him broke into a run. The younger brother reached the gate first, but, apparently by accident, stumbled and fell. The others leaped over him and sped toward the house. But the younger brother as he lay prostrate kissed the earth, that great mother of us all.

No race has ever surpassed the Greeks in their keen sense of oneness with nature. In this legend we see clearly brought out the idea that he who wishes to prosper and go far must recognize this close relation and pattern his life by it. Such a one will in the long run triumph over force and fraud, over human scheming and machination, for as Wordsworth tells us:

To the solid ground
Of nature trusts the mind that builds for aye.

I am speaking to scientific men and women, and I do not need to point out how well this line expresses the mental attitude of the scientific worker; how the answer of nature to experimental question is for all of us as the decision of the Supreme Court, the ex-cathedra utterance of infallible authority. All students of nature, whatever their special field, are agreed in the acceptance of

[1] Published by permission of the Director of the National Bureau of Standards of the U. S. Department of Commerce.

this fundamental principle. We all trust nature as the solid ground beneath our feet.

But while nature is one, and her students are at one in their complete loyalty to her, these students group themselves, broadly speaking, into two classes: those who study only the physical aspect of nature, the astronomers, the geologists, the chemists and the physicists, and those who include in their view the phenomena of life, the paleontologists, the biologists, the physiologists and the physicians.

The division between these groups is marked by one of the very few sharp lines of demarcation to be found in nature: the distinction between the animate and the inanimate. Other apparently sharp lines there have been in the past, notably the distinction between organic and inorganic chemical compounds; but with the advance of our knowledge these have disappeared, leaving the life line as perhaps the only one which can still be regarded as tenable. It may be that in time this too will disappear, but for the present it stands unassailable.

The aspect which nature presents differs markedly as we view her from one or the other side of this line. To those who have eyes only for the inanimate she presents a certain definite appearance; to those who study the manifestations of life her aspect is quite different; while to the philosopher, whose aim it is to view all possible sides of a question, nature's aspect is perhaps the most interesting and instructive of all. Let us look at nature through the different glasses worn by these three types of students.

As the student of the physical side of nature goes farther and farther with experiment and observation there grows within him a deeply rooted feeling of satisfaction which is the all-sufficient reward for his devotion. Man may prove false, but to nature he may trust for aye. For this feeling of satisfaction several contributing causes may be assigned.

The first is the conviction, confirmed by every new experiment, that in the last analysis nature is reasonable. Her ways of working as we uncover them are found to be throughout consistent with the laws of thought. She never demands of a student that he subordinate his reason; she rather appeals to it. True, he learns from her to think more accurately and broadly as he learns to grasp more and more of her complexity, and he realizes that all that which he has learned or can ever hope to learn is perhaps but an insignificant fragment of nature's complete structure; but his feeling of satisfaction becomes the deeper as he reflects that whatever may remain unlearnable this much has become plain; that he himself is a part of nature; that her laws are his; that in learning the language which nature speaks he is but learning his mother tongue, beside which the Babel of confused philosophies which man has affected in all ages is but as infantile prattle.

A second quality of nature which contributes to the satisfaction which she imparts is her perfection. Nature, from the physical point of view, is our ideal of a perfect machine. The revolution of the earth on its axis is our ultimate standard of time by which the most perfect clocks must be corrected; and if there are indications that even this motion is being very slowly retarded it is from some other standard furnished by nature herself that we are able to verify this fact. As a perfect straight edge nothing of human construction can hope to equal the path of a ray of light. Even though fire should destroy or thieves break in and steal all the meter bars in existence, this standard of length could be reproduced from a light wave. Efforts to scatter or diffract X-rays by the most finely ruled gratings that could be made were failures; only when re-

course was had to the much more finely spaced structure provided by nature in crystals did this become possible. Nature is the envy and the despair of the instrument maker.

Sir John Herschel speaks of the perfection and uniformity of the atoms, comparing them to manufactured articles. True, his concept of the atom was not that which we have to-day, yet in its essentials his comment still holds good; and with the complex nature of the atom as we now understand it this uniformity and perfection are but the more remarkable.

A third quality is ease of working. Perhaps this appeals most strongly to the mathematician or the astronomer. By means of the most powerful methods known we can scarcely attain an approximate solution of the problem of three bodies, yet this and far more complicated problems are daily and hourly handled by nature without the slightest hesitation in the motions of the heavenly bodies and in the atom. The terms "simple" and "complicated" seem meaningless to nature. Infinite is her grasp; multiplicity of conditions is to her as nothing. The machine, however loaded, works without creaking. Moreover, nature's laws seem to have a way of executing themselves without needing enforcement. You may have been fortunate enough to have been a pupil of one of those rarely gifted teachers who seem to manage without effort. Not a word is required. Her mere presence suffices. And so Dame Nature, like the catalyst of the chemist, brings about the reaction without perceptible exertion.

Perhaps the most striking illustration of this ease of working is found in the formation of crystals, especially when this takes place instantaneously from a supersaturated solution. At one moment there are billions of molecules milling about among one another at random in a disorganized, liquid condition; at the next, order and discipline prevail. Each molecule has found its proper place and has taken it with accuracy and rapidity.

Just how difficult is this task can best be understood by trying it. During the second Jubilee of Queen Victoria one of Her Majesty's ships of war, stationed at Alexandria, staged an impressive performance in honor of the occasion. The ship lay at anchor at such a distance off shore that a network of ropes hanging over her side was invisible. Her crew, in holiday white, lined the rail. At the signal of a whistle all was activity. The watchers on shore saw a multitude of white specks swarm over the rail and down the ship's side. A few seconds of confusion, then quiet and order. The specks ceased moving; the ship's guns boomed out the royal salute, and there, outlined against the gray side of that symbol of Britain's sea-power, the watchers read the words: "God save the Queen!" formed by the white-clad bodies of her loyal subjects.

This feat was doubtless difficult enough to require much practice, yet compared with what nature accomplishes in the formation of a crystal it is quite simple. Each sailor had his allotted place and went to it in an orderly and prescribed fashion. But suppose there had been no allotment of positions, and the thing had to be done on an interchangeable basis, a man to every place, but no particular man to any definite place. What confusion and delay would have resulted! Several men would make at once for the same position; another position, equally important, might be neglected. Yet this is the task which molecules have to accomplish in crystallizing. No place is reserved for any particular molecule. The whole process may be repeated as often as desired with a different permutation of the actors and in an incredibly short time.

A fourth and no less remarkable quality of nature is the permanence which she frequently exhibits on her physical side. Maxwell, in the article "Atom"

which he wrote for the ninth edition of the Encyclopedia Britannica a generation ago, philosophizes on the permanence of the atoms as they were known in his day. He compares the active existence of an atom of hydrogen, subjected to the ceaseless round of chemical change in its environment, with the experience of living organisms, and points out that there is an essential difference between the two. The organism, as a result of change in environment, may in many generations undergo a process of development which we call evolution and finally assume a form quite different from that which it had at the beginning, while hydrogen, that may have been occluded billions of years ago in a meteorite and that has existed there ever since in monastic seclusion in an unchanging environment, is found on liberation to possess properties identical with those exhibited by its brothers who have lived the active life of chemical change for the same long period.

The discovery of radioactivity and the breakdown of atoms in no way invalidates the essential principle of this argument. In Maxwell's day it seemed that though molecules broke down atoms were permanent; in our time we go but a step lower: atoms may break down, but their constituent protons and electrons appear unchangeable.

Still a fifth quality of physical nature is economy of effort. This is called by the physicist the principle of least action and is illustrated by the fact that the path of a ray of light from a point in one medium to a point in another is that which requires the least time of travel.

Jointly these qualities present to us a picture most satisfactory to the mind. What nature does in her physical activities she does well, thoroughly, economically and always reasonably, though ever mechanically. To watch the physical working of nature is like listening to the music of a player-piano, which by its very perfection of time and execution unimpaired by any fatigue due to ceaseless repetition, undisturbed by noise and chatter in the room, irresistibly suggests the unconscious, impersonal machine, rather than the sentient, fallible and perhaps temperamental living artist. A machine, impersonal, mighty in power, infinite in complexity and altogether overwhelming in its entirety, yet inviting our confidence by its reasonableness, its rationality as far as we are able to follow its workings—such is the aspect of nature as seen by the student of her physical phenomena.

But when we include the element of life in our study of nature her aspect changes. This, one may say, is only to be expected. Life itself is so transcendental a phenomenon, so different in kind from anything with which the physical student has to deal, that it is but reasonable to expect nature's aspect to differ when viewed from this new position. True enough; we expect something different, but something still possessing those qualities of perfection and ease of working which physical nature exhibits, perhaps exalted to a degree unsuspected before; but we are hardly prepared for that which animate nature actually shows us.

The first impression which she produces upon us is one of greatly increased complexity. Chemistry, it has been said, is nothing but complicated mechanics, a statement which the modern atomic theory, a microscopic edition of celestial mechanics, abundantly confirms. Physiology likewise we may perhaps regard as complicated chemistry, and psychology as complicated physiology. But mere complexity, of whatever high order, has no terrors in itself for the scientific student; it rather acts as a challenge. Were this all, the aspect which nature presents to the biologist would differ only in degree from that which she exhibits to the physicist.

But with life a new element demands

our consideration, for life is accompanied by consciousness, and in its train follow choice and free will, things so hard to reconcile with mechanical action that some have even argued that they do not exist. And what is still more serious, it is impossible for us, as nature has shaped and endowed us, to regard phenomena involving life and consciousness without introducing ethical considerations, the effect of which upon the picture is the most potent of all.

Living nature we find still reasonable. She violates none of the laws of thought. At times she holds rather tenaciously to logic when we, perhaps, would fain see her exhibit a little sentiment. But of the other qualities that contribute to the satisfaction of the physical student not one escapes without qualification.

When we examine animate nature for that perfection which was so prominent a quality in her physical aspect we are rather puzzled by what we find. We see much admirable perfection, almost perfect adaptation to environment; in other places nature seems to have grown weary in well-doing and, so to speak, skimped the job. Marvelous as is the human eye, exquisitely as it is planned, the practical execution of this plan impresses the critical student as having been rather incompletely carried out, the resulting product too often requiring the assistance of human skill. You will recall the saying of Helmholtz that if an optician were to furnish him an instrument containing as many defects as the human eye he would send it back to him with his severest censure. Here and in other instances animate nature seems to have abandoned that high ideal of perfection which was so characteristic of her physical aspect, and to be satisfied with something that is "good enough," that works after a fashion.

Again, we sometimes find living creatures encumbered with useless or even undesirable vestiges of organs which served at one time, it may be, some useful purpose, but which now lag superfluous on the stage of time. Such is the appendix in man, always a potential and often a real source of danger. "Why," asks the student, with the perfection of inanimate nature fresh in his memory, "why does not nature promptly remove the useless? What estimate would be placed upon the intelligence of a factory superintendent who would allow a discarded piece of machinery to remain in its place until natural decay removed it?"

Gone, too, is much of nature's ease of working. The machine creaks badly at times, especially in the newer and higher orders of life. Nothing is more important for the preservation of a species than the birth of a new individual, yet nature, in the human type, loads this important function with difficulty and danger.

As to the attribute of permanence, there is perhaps no quality so alien to life in all its manifestations. There may be found a few organisms which have retained their ancient forms for long geological periods, but for the most part the law of life is change; if not evolution, then degeneration.

It is not to be denied that this continual change has an attractiveness of its own for the biological student which may fairly be set off against the satisfaction afforded the physical student by the permanence which nature shows him. But this process of evolution, in the methods by which it is carried out, involves a very important consideration which never comes to the attention of the physical student: a quality which sets the ethical sense of man hopelessly at odds with Mother Nature, who has bestowed it upon him.

For nature is cruel or at least icily indifferent to the existence of suffering in her domain. She has no pity; mayhap she has given it all to man. From top to bottom of animate nature the weakest go to the wall. Her highest ideal seems to

be that might makes right. And the particularly irritating feature of it all is that this procedure of hers is so coldly logical. As a working plan it is unassailable. Ethical considerations assume by comparison the aspect of impracticable theories.

Again, nature is wasteful, especially in the lower orders of creation. The physical principle of least action disappears completely. Millions of fish eggs are spawned and left to chance survival. Rain falls plentifully in the ocean when it may be sorely needed by living creatures on land. Notice that the element of waste in this process appears only when the life consideration is introduced. From the purely physical side the less rain on land the better, as rain on the continents effects their slow removal and ultimate disappearance. Nor does animate nature seek the shortest and most direct path to her ends. Her method of advance seems rather to be one of trial and error; she makes a thing, tries it and improves it for an eon or so, and then rejects it to try another line.

As man views nature in this aspect his impression is one of bewilderment. He wonders if he may trust his vision, if he sees aright, for there comes to him a strange suggestion: that he, a child of nature, because of this new ethical sense with which she has endowed him, is by just so much nature's superior. He disapproves, yet is helpless.

He looks back into the past of his race and sees that his ancestors were rather more closely in harmony with nature in this respect than he is to-day. Man has long since abolished attaint, but nature still visits the sins of the fathers upon the children. Human law no longer countenances the rack, but tetanus still tears the muscles of its victims from their very fastenings. Our law holds that it were better that nine guilty should escape than that one innocent should suffer, but nature's punishments are distributed with the blind impartiality of chance. It is considerations such as these that engender in the mind of man that strange suspicion of his own superiority which he half recognizes, half hesitates to believe.

But again he reflects grimly that every adolescent passes through a stage in which he thinks that he knows more than any adult in existence. Is this our own position? Is the human race intellectually still so immature as to exhibit a similar behavior toward Mother Nature?

Under the influence of such reflections the student of nature often keeps his own counsel, fearful lest an expression of opinion, however hesitatingly and modestly put forth, should place him in some such ridiculous light before his fellows. He keeps silence, waiting and hoping for clearer vision, and in the meantime in a sadly bewildered state of mind.

But the poets, to whom we scientists must so often turn when expression fails us, have not been blind to this aspect of nature, nor have they held their peace. Browning puts this answer into nature's mouth:

> "See
> Or shut your eyes," said nature, peevishly.
> "It nothing skills, I can not mend my case."

And Tennyson complains

> That nature lends such evil dreams.
> So careful of the type she seems,
> So careless of the single life.
>
> So careful of the type? but no.
> From scarpéd cliff and quarried stone
> She cries, "A thousand types are gone:
> I care for nothing, all shall go."

There can hardly be a sharper contrast than that exhibited by these two types of students of nature, the physical and the biological, the one satisfied, the other bewildered. The cynic would say, perhaps, that both lack a proper view; the physicist does not see far enough to disturb him, and the biologist sees too much for his own peace of mind.

Now comes the philosopher, whose pro-

fessed object it is to try to view as broadly as possible and in true perspective all different varieties of human thinking, and so far as possible to coordinate them. Here is an excellent opportunity for him. Has he anything to say that will be helpful in this *impasse* in which we find ourselves?

Yes, he has something to say; trust a philosopher for that; but as to its helpfulness, we may perhaps reserve our decision until we have heard it. And what he begins by saying does not seem to have much application to the difficulty. But philosophers have their own way of saying things, and must be humored if they are permitted to speak at all.

"You know," says he, addressing himself to the physical student, "that matter exists in three states, solid, liquid and gaseous. Warm up a solid. For a long time, perhaps, it remains a solid, its appearance and behavior changing by insensible gradations. The continual addition of heat is accompanied by a steady rise in temperature. But sooner or later a critical point is reached. Though the influx of heat is not halted the temperature stops rising. A new effect is produced, different in kind from any phenomenon previously exhibited. We say the body is undergoing a change of state and becoming liquid. In this new state new laws govern its behavior; its properties are radically changed. Yet it still remains matter, of the same chemical composition as before, in substance unaltered, though in behavior transformed.

"Soon the transformation is complete. The temperature again begins to rise as the influx of heat continues, and the properties of the liquid alter by insensible gradations.

"Eventually another critical point is reached, signalled by another halt in the rise of temperature. The liquid begins to boil, assuming again new properties. Yet in this new gaseous state it still remains matter, of the same composition as before, in substance unaltered, in behavior transformed.

"Now," continues the philosopher, "we may say that nature has been steadily warming up to her work since the beginning of things. Her past history has been one of change, of growth, of that development which we call evolution. Her future, if hindsight is to be trusted, will carry this evolution onward to a consummation of which we can as yet form no conception. And in this warming up process we may distinguish certain critical stages, not unlike the changes of state of matter. One of these critical points occurred when life first made its appearance, a totally new phenomenon superimposed upon inanimate nature.

"For untold ages no life was possible on this old earth of ours; it was too hot to permit it. But eventually, when conditions allowed, life appeared. With its appearance new phenomena present themselves. The machine-like character of nature loses its perfection and ease of working. New motives of action are recognizable; new combinations are possible. Yet living matter is but matter. In acquiring life it has not changed its fundamental character. In substance it is unaltered; only in its behavior is it transformed.

"Moreover, this transformation has not been complete. Living and non-living matter exist side by side, and will probably continue to do so. The physicist would call this the co-existence of two phases at one temperature, like a mixture of ice and water at the freezing point, each following its own laws and exhibiting its own characteristic properties.

"We may, perhaps, by poetic license think of the first beginnings of life as feeling strange and lonely in the midst of the non-living matter surrounding them, so different in appearance, in behavior. And perhaps we may imagine that the works and ways of non-living

matter occasionally grated on the sensibilities of the living, and called forth the protest: 'Why are you so mechanical? Why not show a little flexibility occasionally?' But this protest, if any such were made, was wasted. "It is my ancient way," replied non-living nature, "the way I did for millions of years before you new-comers appeared upon the scene. I can not mend my case. Why not do as I do and be sociable?"

"But this is just what living matter will *not* do. Like white men in the tropics, it maintains its own standard of living among an overwhelming majority of an inferior grade of civilization. Millions of years have passed. Life is no longer a new-comer, a feeble colony, but has waxed mighty, and has become the outstanding feature on the earth's surface.

"And now," continues the philosopher, "we have come to a second critical point. Life has reached such a degree of development that a new set of phenomena is beginning to make appearance, something different in kind from anything that has been before; as different in its turn as was life itself compared to inanimate matter; something superimposed upon life as life of old was superimposed upon the non-living. And it is, appropriately enough, in man, the highest type of life, the flower of creation, 'the heir of all the ages,' that this new thing first makes itself manifest—a moral sense, an ethical feeling, which often finds itself as much a stranger in its environment as life must have felt among the crystals and colloids in which it began its existence. If we must find a single word to express this new quality, let us call it Soul."

The philosopher now addresses himself to the abashed and bewildered biologist. "Take courage," says he. "Your vision is better than you realize. Your face is turned toward the light, though for the present you fear to look at it, or can not bear its glory. Allow time for your sight to become accommodated, and then look boldly at what you see.

"You feel that certain aspects of nature antagonize your ethical sense, but you hesitate to commit yourself to this position lest you should repeat the folly of the adolescent who holds his elders in disdain. But you are not an adolescent. Look closely.

"There is perhaps nothing more characteristic of conceited youth than its cocksureness. The explanation of things is so clear to him that those of his elders who for some incomprehensible reason can not see for themselves had much better ask him about it. Why question and experiment? It is as plain as the nose on your face that it must be so and so. Common sense tells you that —if you have any!

"We reach our intellectual majority when we begin to realize that it is not quite so easy as all this; when we begin to question anew, as we did in childhood; when we become increasingly reluctant to commit ourselves to an opinion.

"Now the human race, as represented by its intellectuals, has passed through something strangely like this stage of adolescent conceit. There was nothing more characteristic of the thought of the Middle Ages than this same cocksureness, this disinclination to put questions to nature, this predilection for spinning explanations out of its own inner consciousness. Why look through Galileo's telescope to see what he calls 'the moons of Jupiter'? Are there not seven planets known? And is not seven a perfect number? There can be no more planets; it is a waste of time to look for them. And as for light bodies falling as fast as heavy ones, why, common sense would tell you differently, even if Aristotle had not done so. Who is this upstart that he pretends to know more than Aristotle? And of all presumption—to assert that he can see spots on the face of the perfect sun!

"But man is beginning to emerge from

this mental state, and is entering upon his intellectual manhood. You students of animate nature are in the forefront of the advance. You question, you observe, you experiment; you are hesitant about forming an opinion. You have reached years of discretion. Trust your vision, your reason. If something within you suggests that your ideals are superior to some things which you see in the established order of nature, if you, too, see spots on the sun, fear not, neither be ashamed. Ideals can rise higher than their source. Just as every great genius had parents of less than his own mental ability, who yet in some mysterious way endowed him with more than they themselves possessed, so nature has produced within us something without precedent in the life history of the earth. And as a parent watches with pride a child who gives early promise of outdistancing his elders, so Mother Nature may be watching us.

"It is all very interesting and amusing to sit, as it were, in a box at nature's theater and watch the Pageant of Time; to see life slowly and perilously begin; to watch it wax and develop to incredible complexity. But now the call comes to you; your cue is spoken. Within you is developing a new thing, as wonderful as life itself, and no less rich in possibilities. Life in its turn has brought forth something of a higher order, transcending itself, as it once transcended non-living nature. And that this new thing has elected to make its appearance in and through you, the highest of nature's children, what is more reasonable? Do men gather figs of thistles?

"What is this new thing which nature has brought forth, and with the development of which we have been entrusted? No man can say, but it is a fair inference that it will go far. Life has gone far from a tiny speck of protoplasm; who knows to what length this new thing, this mind, this soul, if you will, may carry us? But with all that we are, with all that we may become, we must ever bear in mind that nature is our mother, that in all that is fundamental we are one with that which we see about us; in substance the same, differing only in behavior."

ARTIFICIAL BEACH CONSTRUCTION IN THE VICINITY OF NEW YORK

By HENRY S. SHARP
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Although millions of dollars have been spent in the past few years to build beaches in various parts of the United States, the idea that beaches can be constructed artificially is strikingly novel to most people. Yet in recent years the growing popularity of the shore as a vacation resort has so emphasized the value of good beaches that an increasing number of municipalities, clubs and private individuals are seeking to better their bathing facilities by constructing artificial beaches. The success of these attempts has varied as widely as their cost.

As a rule the most successful beaches have been built by engineers thoroughly conversant with local physical conditions, who have provided structures to counteract the effects of these conditions or to shape them to their ends. Conditions vary so widely from place to place that rule-of-thumb methods are sure to give a large percentage of failures, and a structure successful at one place may be a dismal failure at another. On the other hand, the engineer who wishes to attack his problem scientifically finds that science has done very little to help him. He is almost entirely without trustworthy facts, and must work up his data from hasty studies of his own. Unfortunately the conditions of competition under which most beach contracts are awarded do not allow much expenditure of money for experimental purposes, so the builder is forced to work with imperfect information, with the result that many beaches never attain their greatest possible success.

At the suggestion of Professor Douglas Johnson, of the National Research Council's Committee on Shoreline Investigation, the writer made a study of certain artificial beaches in the vicinity of New York, in order to get a better idea of the magnitude of this type of engineering work and to discover, if possible, some of the physical conditions which help to determine the success or failure of man's attempts to build himself artificial seashore playgrounds. The results of such a survey are immensely impressive if one considers only the cost of such structures; while a double interest exists for those who have some curiosity as to how waves build up and tear away the sand along the shore. The city of New York has spent over $2,000,000 in building its public beach at Coney Island. Wrightsville, N. C., an incorporated town with a winter population of twenty-two persons and a summer population of five thousand, has spent $60,000 in building jetties. At Atlantic City jetties constructed on an extensive scale have given satisfactory results. Thus, from large to small, the examples of beach construction finished or under way might be multiplied extensively.

In making his studies the writer was generously aided by more engineers and other competent authorities than he can name in a short article, and he wishes to express here his indebtedness to them and to their published accounts of special shore works.

Agents of Erosion and Accretion

An analysis of the work of erosion and accretion performed upon beaches will show that the chief agencies involved are waves and currents, usually interacting

to such a degree that it is often difficult to arrive at an estimate of their relative importance. The destructive action of waves is dependent upon their size and character, the direction of their approach and the depth of the water. On a calm day waves approaching the beach are likely to be of the gentle swell type, and if the bottom slopes gently seaward, material should be moved landward to build up the beach. On the other hand, an onshore storm will pile the water upon the beach several feet above normal high tide, so that a strong hydraulic bottom current or undertow flowing seaward may be caused, disastrously eroding the beach. If the wind blows strongly offshore, the surface water will be blown seaward, and a compensating shoreward current coming in along the bottom will deposit sand upon the beach. That this is more than theory is indicated by the Jersey fisherman's axiom that "a west wind builds up the beach."

Another factor of extreme importance in determining the effect of waves on a beach is the depth of water seaward of it. Large waves require deep water; if a beach is protected by shallows offshore, the great waves will break harmlessly where the water first shoals. Only after a long storm will the ceaselessly working waves have removed material enough and deepened the water sufficiently to allow them to attack the shore. Usually the storm is over before this can happen, and the ordinary processes of deposition have replaced the sand before another storm. Thus it happens that one long-continued storm may do more damage than several short ones. When the water deepens rapidly offshore, even short storms are able to throw waves upon an unprotected beach which is open to destruction. It is true that the best protection a beach can have is "plenty of sand," so that it can lose much without being destroyed.

All the sand removed from a beach is seldom permanently lost; usually a large proportion is replaced during the next calm period, or after a change of winds. As an example, the fortunate case of the New Jersey coast may be cited, where the most destructive northeasters are usually followed for several days by steady west winds, rebuilding in many cases a large part of the destroyed beaches. But in places the difficulties of protection are increased by the permanent loss of a large part of the storm-eroded sand; this is true on the Netherlands coast, where the strong tidal currents of the North Sea remove the sand beyond the hope of reclamation.

This interaction of waves and currents working toward the construction or destruction of a beach is typical of the forces with which the marine engineer must struggle. Authorities differ greatly concerning the work of tides on beaches, but it seems most probable that the ordinary rise and fall of tides will have little effect on an open coast. Tidal currents having a velocity of several miles per hour may develop on certain parts of a coast broken by many inlets, bays and headlands.

On the Jersey coast inlets through the barrier beach are marked by two tidal deltas, one in the lagoon and one in the ocean, formed by the inflowing and outflowing tidal currents. The sand in these deltas is permanently removed from beach circulation, and therefore such tidal currents may have a detrimental influence by robbing the beaches.

Wave currents, last to be considered, are the producers of "beach drifting," probably the most important means of transportation by which beaches are built up or destroyed. Beach drifting is caused by waves running upon the beach obliquely and forming a swash which carries beach material in the direction of wave propagation up the beach, whence the force of gravity causes it to return almost directly down the slope. Thus wave after wave results in a net move-

ment of material to leeward by a series of parabolic leaps. The importance of this process is everywhere demonstrated by the accumulation of sand found on the windward side of any obstacle which obstructs the movement of drift along the beach. Many prominent features of our coasts owe their existence to this drifting movement: Sandy Hook and Rockaway Point, outside the Lower Bay of New York, are due to drifting caused by wave currents, and both have been rapidly extended in the direction of current movement.

Shore Protective Devices

Engineering opinion differs greatly concerning the value of artificial defenses used to protect beaches against loss. A limited number have proven useful under such a variety of conditions that no doubt remains about their utility, although there is still much argument about their detailed construction. It may be said that any structure sufficiently tight to interrupt the beach drifting will result in accumulation on the side from which the drift comes, but such effects may be offset by the scour which may be caused at times on their leeward side.

Protective devices are generally of the following types: bulkheads, placed at the rear of the beach and extending parallel to the shore, are made of wood, stone or concrete, and are designed to prevent erosion and scour on beaches inundated by extreme storms. Groins, by which are understood wooden or stone structures usually placed at right angles to the shore and extending into the ocean far enough to break up the long-shore movement of drift, are the most important beach protective device. Many disastrous experiences have shown that groins or bulkheads should seldom if ever be used separately; a system of groins having their landward ends tied to a bulkhead is considered the best engineering practice. Groins designed to secure accretion to the beach are usually built with piles projecting so they may be raised as deposition occurs. When they are exposed to the full effect of the waves they must be protected by massive riprap, or be made entirely of stone; wooden groins will be torn out, as happened at Long Beach during the recent storm of February 22. Groins are usually placed at right angles to the shore, and are spaced variously according to local conditions.

Physical Conditions in the Vicinity of New York

The effect of the wind, the usual cause of shore drifting, may be studied by wind "roses," made by plotting the relative importance of the wind from each quarter obtained by multiplying the hours of duration from each direction by the average hourly velocity. The use of wind "roses" in explaining the direction of beach drifting as a function of dominant winds and coastal orientation is well exemplified by the cases of the southern Long Island and New Jersey coasts.

The northern coast of New Jersey extends in a north-south direction, and drifting is northward, while the adjacent Long Island coast has an east-west orientation, with westward drifting. For a long time these anomalous directions of drift movement confused students of shoreline problems, but the explanation is now known to be very simple and logical. Obviously the only winds capable of producing oblique onshore waves on the Long Island coast are from the southeast and southwest. The wind "rose" for Sandy Hook shows the southeast winds to be less important, and eastward beach drifting due to the stronger southwest winds should be expected. However, winds coming from the southwest have a shorter sweep of open water due to the nearness of the Jersey coast, while the southeast winds sweep in from the open ocean and are therefore strong

enough to bring about westward drifting.

On the north-south New Jersey coast, waves from the northeast and southeast approach the shore obliquely to cause drifting, but although the northeasters are the stronger winds, the coast is partially protected by the proximity of Long Island, and the weaker southeast winds are able to form the stronger waves with very marked northward movement of material along the northern New Jersey coast. A rough diagram will convince any one of the validity of this explanation for the movement of material on these coasts.

The straight linear character of the New Jersey and southern Long Island coasts is ideal for the generation of shore currents moving for great distances in the same direction; on the other hand, the Connecticut shore of Long Island Sound is so irregular that the many minor indentations and prominences exercise a great influence over the direction of beach drifting, and currents setting for a long distance in the same direction are unusual. The difficulties of beach construction on the Connecticut shore are increased because the natural supply of sand is smaller than on the Long Island and New Jersey coasts, where unconsolidated sands and gravels supply to the waves an abundance of material for the beaches, whereas, in Connecticut, a large proportion of the shore is formed of resistant crystalline rock disintegrating to form sand very slowly.

The Coney Island Municipal Beach

The largest and most successful artificial beach on the North Atlantic coast is the New York City Municipal Beach at Coney Island on the southwest point of Long Island just outside New York Harbor. Here, on a front of nearly two miles, the shoreline has been advanced seaward over three hundred feet by artificial means. Before construction started there was a natural beach some fifty feet wide, protected by a few privately built groins, but no systematic protection was afforded so that advance in one place was offset by retreat in another. The natural supply of sand was insufficient to build a beach of the desired width, so that it was necessary to pump sand upon the beach. The chief problem of the engineers was to determine what type of structure would give the greatest stability to the sand thus placed.

Careful study resulted in the erection of sixteen timber groins, each five hundred and sixty feet long, and spaced at six-hundred-foot intervals. All these groins were securely tied to a low bulkhead to prevent scouring in case of exceptionally severe storms and were covered by heavy riprap for their outer two hundred feet.

At present, several years after the completion of the work, many interesting features are revealed. Since the general movement of material on this coast is westward, it is to be expected that all the groins interrupting the drifting would have more sand on their eastern side than on the western. At Coney Island this is true of the eleven western groins, showing that movement on the western portion of the beach is westward. In addition, a large quantity of red sand was poured upon the beach during construction at a point about one thousand feet west of Steeplechase Pier; a few months later this sand was found several thousand feet west of, and more than a thousand feet eastward of its original position. Finally, improved beaches to the east, stable for many years, have suddenly moved forward over a hundred feet since the completion of the Coney Island beach. All this evidence points to an unusual change in the direction of beach drifting, so that east of a certain point on the beach, probably in the vicinity of

the twelfth groin, sand moves eastward instead of westward.

A number of explanations have been advanced to account for this interesting change in the direction of beach drifting, the most promising being based upon a study of the location of Coney Island in reference to the protection afforded against sea winds. As any map will show, the eastern shore of Coney Island is protected by Rockaway Point from the heavy southeast waves which sweep in with full force upon the western portion of the Island. On the other hand, waves coming to Coney from the southwest have only a short fetch, due to the nearness of the New Jersey shore; they are wholly incapable of moving material eastward on the western portion of the beach where the heavy southeast waves break. On the eastern portion of the beach, however, they have a greater fetch than the southeast waves crossing the narrow stretch of open water behind Rockaway Point. Here, therefore, the southwesters are dominant, and cause an eastward movement of shore débris. The vicinity of the twelfth groin from the west is where the domination of southwest waves on the eastern portion of the beach gives way to the domination of southeast waves on the western portion, and hence is the point from which material is moved in both directions. The authorities take advantage of this unusual movement of shore drift to replenish the beach at intervals; they simply pump sand on the shore in the vicinity of groin 12, and the waves distribute it all along the shorefront. This has allowed economical maintenance of the beach, which is considered very successful several years after its completion.

Beaches on the Westchester County Shore

Although conditions of beach construction on the north shore of Long Island Sound are very different from those on the south shore of Long Island, it is still true that the solution of the problem will be found by a proper appreciation of the movement of beach material as governed by the direction of approach of the dominant waves. In making plans for the construction of a beach it will always be found helpful to study other beaches in the vicinity in order to learn what conditions have governed their development. In the course of field work in Westchester County, three adjoining natural beaches, Meadow, Rye and Oakland, were visited, and showed a distribution of sand highly significant of their origin.

Meadow Beach, the northmost of the three, is situated between two small rocky promontories; the northeastern portion of this beach has a broad strip of sand which narrows rapidly toward the southwest until it is only a few feet wide. The cause of this uneven distribution is not far to seek. A short distance offshore are the Transport Rocks, largely protecting the beach from the onslaughts of southeast waves, but to the south and the east the beach is exposed. In this region south winds are stronger than easterlies, so that the dominant waves are from the south. Meeting the northeastward extending shore obliquely, they cause drifting toward the northeast, until the northern promontory breaks up the movement causing the formation of a wider strip of beach.

Adjacent to Meadow Beach on the southwest is Rye Beach, a broad, fine beach, sheltered, as is the former, by two rock promontories. Here again the distribution of sand is uneven, but unlike Meadow Beach the southern portion is broader than the northern. The presence of offshore rocks explains the apparent anomaly. To the south and southeast lie the Forlies Rocks; to the east lie the Transport Rocks, but to the

northeast the beach is exposed to waves coming in over a long stretch of open water causing southwesterly drifting until the interference of the southern rock causes deposition on that portion of the beach.

Oakland Beach is situated a short distance south of Rye Beach, and if the position of the rocks offshore is noted it will be seen that the beach is well protected from north through east by the nearby Forlies Rocks, but to the southeast it is more exposed. Southeast waves should here cause beach drifting in a northerly direction, and the beach should be wide at the northern end, if the theory is correct. In the field one glance shows that the facts conform to theory, and that the northern portion is wider.

Here, then, is a series of three beaches, two of which are wider at their northern ends, while the third, lying between the former, is much broader at the southern end. This irregular distribution of sand is a strong argument against the presence of currents setting for a long distance in one direction such as are found along the Long Island and most of the New Jersey coast. As has been shown, the distribution of sand in each case is explained by the direction from which comes the dominant wind blowing across the greatest stretch of open water. With this in view, it seems evident that on this coast marine engineers should give particular attention to the often slighted, but here very important, process of beach drifting.

Investigations of this character show very clearly how the study of shorelines and shore forms so long classed as one of the "pure" geological sciences has in the last few years taken its place with so many of its sister branches of geology as a subject of distinct practical value. It also indicates how much the lives and habits of the present generation have been influenced by the invention of the automobile; before it became cheap and plentiful the beaches were more than large enough to accommodate all who visited them. Now they are overwhelmed with visitors, and efficient means of enlarging and increasing them are eagerly being sought.

THE SOIL AND THE MICROBE[1]

By Dr. SELMAN A. WAKSMAN

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THE practical benefits derived from the study of the rôle of microorganisms in infectious diseases and of the means of controlling these infections, as a result of the investigations of Pasteur, Koch, their associates and numerous followers, gave rise to expectations of similar results in other fields, where the transformations are known to depend largely upon the agency of microbes. However, these expectations were not always fulfilled, and frequently the results obtained gave little promise of practical exploitation. This is especially true in the application of the results of studies of numerous microorganisms which inhabit the soil. If the primary purpose of scientific investigation is to explain known facts and common practices, then the study of the biochemical processes carried on in the soil by the complex population may have justified itself. However, if the primary purpose of research is to improve or change common practice, then the fifty years of study of the bacteria and other microbes of the soil has with certain exceptions, such as the use of cultures for soil inoculation and a better understanding of the employment of green manures and the use of stable manure, largely failed in its goal.

Far from agreeing in the interpretation of the results obtained, outstanding investigators in the field frequently dispute the very activities ascribed to the great majority of soil organisms and even question whether any of the organisms commonly studied have been definitely shown to be soil forms and the causative agents of known processes. Soil microbiology represents a good illustration of a science which is quite dependent upon information in related sciences.

Since the importance of microorganisms in soils is interpreted in terms of the influence of these organisms upon growth of higher plants and since the complex physical and chemical properties of soils are so closely related to plant development, a knowledge of the factors affecting plant growth and of the physical and chemical characteristics of soils is indispensable to applications of findings in soil microbiology. To be able to understand the reason for the crop-producing capacity of the soil, the processes of soil treatment and fertilization, it is not merely sufficient to know the nutrient requirements of the plant, but it is essential to understand the factors affecting the liberation and retention of these nutrients in the soil, processes which are closely related to the activities of the soil microbes. These microbes have become too quickly popular as a result of a few partly justified and many largely unjustified expectations. Many are found who are willing to exploit for selfish purposes this interest of the practical man in the microbe, an interest so well blended with a lack of accurate knowledge. A soil process whose explanation is obscure may be called a result of the action of soil bacteria with little or no knowledge of the importance of such organisms in the reaction. Even the teacher and agricultural expert utilize this explanation, when one is needed and none is available; the mi-

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crobe in the soil has served too frequently for explaining phenomena which were not understood.

What do we know of these soil microbes? Can we ever expect that an extension of our knowledge will lead to a modification of soil treatment and help to change this art of handling soils into a science which will take its place with other biological sciences. It may be somewhat exaggerated to call soil science a biological science, especially in view of the secondary considerations given to soil organisms, as compared with the mechanical, physical and chemical analysis of the soil, but it will be recognized sooner or later that the soil is a biological system, modified by the physical and chemical environments just as other biological systems are.

The foundations for the knowledge of the microbes of the soil were laid by (1) chemists, who have studied certain soil processes, such as the formation of nitrates or the fixation of atmospheric nitrogen; (2) by bacteriologists, botanists and zoologists, who developed methods for the isolation and cultivation of bacteria and other microorganisms, and finally (3) by practical agronomists, who studied plants in their natural environment, the soil. It is sufficient to mention the names of such brilliant chemists as Berthelot and Müntz, in France, of Lawes and Gilbert, in England, of Sprengel and Liebig, in Germany; bacteriologists, like L. Pasteur, F. Cohn, R. Koch; agronomists, like Kette, Rosenberg-Lipinsky, Dehérain and others. Although by 1880 considerable information had accumulated concerning certain soil processes and their importance in plant nutrition, very little was known of the causative agents. The soil was supposed to harbor microscopic forms, but these were very little understood.

The development of the gelatin plate method in 1881 by R. Koch enabled one to obtain bacteria from the soil. The application of this method by Koch and other bacteriologists demonstrated the presence of millions of these organisms in every gram of soil. This striking observation aroused general expectations that soil practice would soon be revolutionized, just as was the case with the practice of medicine. Unfortunately, however, one could find at the very beginning of these investigations the seeds of future disappointments. First, the investigators, especially in Germany, were either medical men who were little interested in the soil as a biological system, but were searching for organisms that might be causative agents of animal diseases and not for the organisms which were concerned in the important soil processes; or they were practical men, interested in the modification of the soil economy rather than in the development of a new science. Secondly, since only bacteria were at that time recognized to be concerned with animal diseases, bacteria were sought in the soil and bacteria were counted, thus giving rise to the name of the whole science under consideration as "soil bacteriology," overlooking thereby the fact that the soil harbors numerous other organisms which take an active part in the various processes. Finally, the plate method of counting bacteria, being fairly well adapted to counting known specific bacteria, gave a wrong impression of the occurrence and abundance of the bacteria in the soil, since it allowed the development of very few types of organisms. The great majority of the soil bacteria and especially those known to carry out certain important soil processes, being very selective in their nutrition, did not develop on the plate, so that where hundreds of millions occur per gram of soil only millions or even fewer were counted.

Soon after the work of Koch there appeared one of the most important contributions from the applied point of view. In 1885–1886 the studies of the

rôle of bacteria in the fixation of nitrogen by leguminous plants were brought to a final successful conclusion. From the times of the Romans it was known that these plants could thrive very well on soils where other plants failed. It was later shown that they could develop on soils receiving no additional combined nitrogen, either in organic or inorganic forms; they were known to leave the soil not poorer but even richer in this valuable element, which is indispensable for the growth of all plants and which is the most costly of all the fertilizing elements supplied to the soil. These plants were found to contain nodules on their roots, the nodules being full of bacteria. Hellriegel and Wilfarth, in Germany, followed by other investigators, established beyond any doubt that the leguminous plants are capable of obtaining the nitrogen from the abundant supply of gaseous atmospheric nitrogen and that the presence of the bacteria in the nodules is absolutely essential to this process. When the bacteria are eliminated, the leguminous plants behave like all other plants and require additional nitrogen. When the soil in which the plants are grown is inoculated with the bacteria in question, the plants begin to grow very rapidly, as when available nitrogen is added. Whether the bacteria themselves fix the nitrogen or whether they enable the plant to do that by some unknown process, was a question of some discussion, although the evidence seems to point quite definitely to the first theory.

The period between 1890 and 1902–1904 abounded in some outstanding contributions to our knowledge of soil microorganisms. Two investigators, who have already made some important contributions to microbiology, namely, S. Winogradsky and W. M. Beijerinck, focussed their attention upon organisms related to some specific soil processes and succeeded in unravelling a number of processes until then only little understood. These investigators developed new methods of attack, which enabled them to isolate organisms that could not be isolated by the ordinary bacteriological methods. These forms were shown to be concerned with certain known processes and were studied in attempts to explain the nature of the processes themselves. The enrichment culture method and the use of synthetic media, some of which were highly specific, allowed the isolation of various new microbes. It is sufficient to mention the organisms responsible for the formation of nitrites and nitrates (it is this latter form of nitrogen which is largely used by the higher plants), the fixation of atmospheric nitrogen in the absence of leguminous plants, under aerobic and anaerobic conditions, the oxidation of sulfur and its compounds and the reduction of nitrates to atmospheric nitrogen.

During this period other investigators made numerous contributions some of which are of outstanding importance. Attention need only be called to the investigations on the rôle of microorganisms in the decomposition of proteins with the liberation of ammonia. This is a reverse process of that carried out by higher plants, and it results in retaining the important element nitrogen in constant circulation in its combined form, of which there is only a limited supply in the soil. Other studies were concerned also with the decomposition of celluloses and other organic complexes, with the liberation of the carbon in the form of carbon dioxide, which becomes available to the growing plant.

All these studies increased the information on the nature of the organisms concerned in some definite soil processes; as a matter of fact, only organisms were looked for which were responsible for processes already known. However, the investigations tended to be isolated phenomena and, although they contributed to the accumulation of knowledge concerning certain represen-

tatives of the soil population, they did not tend to bring to light the various interrelationships of the complex soil population. These microbes live in an extremely complex physical and chemical medium, the soil. They exist in the soil solution and to a greater extent on the organic and inorganic colloidal film surrounding the inorganic particles which go to make up the soil. They carry out numerous reactions, utilizing the products of one another, antagonizing one another directly or indirectly, working frequently in symbiosis, producing substances injurious to themselves or to others and frequently serving as food to other organisms. A single process carried out by a single organism gives only a suggestion of the course of the same process in the soil or of the rôle of the particular microbe in this process when accompanied by numerous other processes, in the presence of a very complex population of fungi, bacteria, actinomyces, algae, protozoa, nematodes, etc.

Such considerations and a desire to study certain transformations in the soil in the presence of the mixed population, rather than by specific forms in pure culture, called forth about 1902 two new methods of study of soil biological processes:

(1) Some investigators limited themselves to the determination of a single transformation or group of transformations brought about by the whole soil population. The soil was at first added to a solution containing a definite substance, later the substance in question was added to the soil itself. After a certain brief period of incubation, an analysis was made of one intermediary or final product, which could serve as an index of the transformation brought about by the microorganisms. If the substance used was a protein, a protein derivative or a complex organic substance rich in proteins, ammonia was usually determined, with the assumption that the more active the microbes are in the particular soil the more ammonia is produced in a given period of time, as a result of the decomposition of the protein. If the substance in question was an ammonium salt, nitrate was measured as the transformation product, with similar considerations. If the fixation of atmospheric nitrogen was studied, an excess of an available source of energy, like mannitol or glucose, was used. Certain correlations were actually obtained between the fertility of the soil and the rapidity of transformations thus brought about, either in solution or in soil, resulting from the activities of the soil microflora and microfauna. It was thus argued that the more active a soil is biologically the more fertile it is; nothing definite was known as to whether one was a result of the other or whether both were influenced alike by the same soil conditions.

(2) The dilution method, suggested by Hiltner and Störmer, in Germany, and Chester in this country, consisted of counting the abundance of certain physiological groups of bacteria, capable of bringing about certain definite transformations.

The second method was utilized only to a very limited extent. It was the first method that found extensive application, due largely to the simplicity of the procedure and to the fact that a knowledge of the complex soil population was not required. The study of the complex soil population and its activities was thus reduced to a few simple manipulations, which could be carried out readily without any expensive equipment and without any extensive information concerning the numerous microorganisms inhabiting the soil and the complex biochemical processes carried out by these organisms. Soon every agricultural institution in every civilized country was devoting full or part time of one or more experimenters to a study of soil processes, using the above-mentioned manipu-

lations, repeated manyfold under various conditions and in various manners. The general impression prevailed that whatever was to be known of soil microorganisms and their rôle in soil processes was already known and that it merely remained to utilize the methods thus developed, just as so many methods in analytical chemistry.

A considerable literature dealing with results obtained by these methods has accumulated. Some of this information helped to advance our knowledge concerning certain soil processes, largely the transformation of nitrogen in the soil, the use of stable manures and green manures, the use of lime and other questions related more to practical soil management than to the solution of the fundamental problems of soil science. The above methods of investigation of soil processes could not advance appreciably our knowledge of the fundamental soil processes, due to the fact that neither the organisms concerned nor the numerous intermediary processes were considered sufficiently and the evidence accumulated could only be circumstantial and of local application. This was largely the reason that, within a decade and a half, there was a practically complete cessation of studies based on solution or soil methods and a considerable reduction both in the number of experimenters and in the literature on soil transformations.

The same period (last two decades) witnessed a growing interest in the other members of the soil population, especially the fungi, actinomyces and algae, belonging to the plant kingdom, and the protozoa and nematodes among the animals.

One of the most interesting ideas, which served as a decided stimulus to the study of the complexities of the soil population, was the protozoan theory of soil fertility proposed by Russell and his associates in England. These investigators limited themselves to the existence in the soil of only two groups of organisms, namely, the protozoa and the bacteria. In their investigations only one biological process was studied, namely, ammonia accumulation in soil. Treatment of soil with volatile antiseptics and dry or moist heat, known as partial sterilization, was known to result in an increase in the fertility of the soil. This increase in soil fertility is accompanied, on the one hand, by an initial reduction followed by a very rapid increase in the numbers of bacteria and an accumulation of ammonia nitrogen, and, on the other hand, by a partial if not complete destruction of the protozoa. Since protozoa are known to feed upon bacteria, the idea was suggested that, in normal soils, bacterial development is kept in check by the protozoa. When these animals are destroyed by partial sterilization, the bacteria develop unhindered and bring about an extensive decomposition of the soil organic matter, thus liberating the valuable element nitrogen as ammonia, which leads to a more extensive crop growth.

Russell and associates thus succeeded in developing a unique theory of soil fertility on the basis of the interrelationships of the protozoa and the bacteria.

This theory led to extensive studies of the animal population of the soil and its relation to other groups of soil microorganisms. The information obtained as a result of these studies can be summarized under three headings: (1) In addition to the protozoa, various other groups of soil organisms, especially the fungi, were also found to be eliminated by partial sterilization of soil. An active competition was found to exist between the fungi and the bacteria for the available energy in the soil, as represented by the soil organic matter. (2) Treatment of soil with antiseptics actually does not destroy all the protozoa. (3) When protozoa are added to bacterial cultures, under controlled conditions, the processes brought about by the bacteria are found

not to be injured, but are in many instances even stimulated.

The microbes of the soil form so complex a population and take part in so many activities that it is futile to attempt to explain the various intricate soil processes by a simple theory based upon the interaction of two constituent groups of the soil population; it is further unlikely that one simple transformation product can explain the complex series of processes resulting from the interaction of the mixed soil population.

One acre of soil contains two million pounds, on the basis of only the upper six and one half inches of the soil. A pound contains four hundred and fifty grams. One gram of an average field soil contains:

One hundred million to three billion bacteria, out of which only one to fifty millions actually develop on the plate.

Ten thousand to one million fungi, representing both fungus spores and pieces of mycelium, the latter being especially abundant in forest and in acid soils.

Ten thousand to twenty million actinomyces also represented in the soil by spores and mycelium.

Ten thousand to one million protozoa represented in the soil by the flagellates, amoebae and ciliates.

Many thousands of cells of algae, including blue-greens, grass-greens and diatoms.

Numerous nematodes, rotifers, insect larvae, etc.

Among the transformations carried out by the bacteria, fungi, algae and actinomyces, it is sufficient to mention:

Decomposition of celluloses, pentosans, proteins and other constituents of the natural organic matter added to the soil in the form of plant and animal residues.

Assimilation processes, resulting in the building up of complex organic compounds by the numerous microorganisms, using the nitrogen compounds and minerals liberated in the decomposition of plant and animal residues.

Oxidation processes, resulting in the formation of nitrites, nitrates, sulfates and various oxidized organic compounds.

Reduction processes, resulting in the reduction of nitrates to nitrites, ammonia, oxides of nitrogen and atmospheric nitrogen; of sulfates to sulfides and the reduction of various organic compounds.

Fixation of atmospheric nitrogen.

Formation of various organic and inorganic acids, which interact with the mineral complexes in the soil, leading to a change in reaction, increase in solubility of insoluble compounds, etc.

The presence of protozoa and other members of the animal population still further complicates the above processes, since these organisms feed on the living and dead bacteria, fungi and algae, as well as on the undecomposed and partially decomposed organic matter, and, later in their turn, also undergo decomposition.

This soil population is so complex, the activities are so numerous, our knowledge is so limited, our methods are still so crude, that we are still unable to construct an intelligent system of soil processes. The opportunities in this field for gaining knowledge, and adding valuable information, which is bound to be of tremendous scientific and practical interest, are great. The soil and the microbe await the investigator, first of all the chemist, the physicist, the biologist, who are not looking for practical gains but for explaining the obscure and observing the unknown. The application will doubtless come.

WHAT IS A POLITICAL ANIMAL?

By Professor EZRA BOWEN
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MAN is by nature a political animal, hence the state. Aristotle, who collected the first zoo, worked up this interesting bit of dogma twenty-five hundred years ago, and down through the centuries from sage to sage it is whispered as the pass-word of political scientists. The standard coin to which the small change of political thought is referred for valuation, it still retains full currency. In text-books, those universal thought-fossilizing mediums, it is usually preserved in original form.

Man is by nature a religious animal, hence the church. Man is by nature a vicious animal, hence vice. Man is by nature ruminative, hence chicle. All as plain as spirit writing—and no more satisfying.

Disraeli, walking in the garden with Dean Stanley, interrupted the good dean's rant against dogma: "Remember, dear dean, No dogma, no dean." Then who can blame the high priests of political science for turning a stony face upon reason and spurning the simple idea that the state is a common law corporation—too simple an idea perhaps for serious pedagogic purpose? Their apology, however, is not far to find—that much they have written out: Simply, in all history there is no record of such a corporation's being formed!

Now a flounder or a sole has both eyes on one side of his head; and biologists have a convenient agreement that it was not always so: that the flounder, travelling continually on one side, with one eye in the sand, found it more convenient to have both eyes on the upper side of him. Yet the biologists, quite as sober-minded and honest as political scientists, have no documentary evidence of a meeting of soles off the coast of Normandy—or of Provincetown flounders—to declare against the one eye to a side idea and for all eyes up all the time. Why, then, should the political scientist require a photostat of minutes showing a resolution, properly moved and seconded, creating a superior will from surrendered fragments of individual free will—this superior will to prevail in any conflict with the individual?

"Am I to believe that the beautiful human eye with the light of the soul shining through was descended from a freckle?" Now we are quoting Mr. Bryan, W. J. Of course we did not expect Mr. Bryan to come to so close a grip with the divine, nor did we expect him to believe that the resultant force of an infinity of circumstances, working over an indefinitely great number of years, produced the lantern of the firefly. "*Whereas,* we the United and Fraternal Order of Night Flies have experienced great inconvenience and some danger. . . . etc., etc. Be it *resolved* that each and every member of the Order be required to carry a small, intermittent, cold light in the posterior portion of his person, etc., etc." No, Mr. Bryan, we can not produce that evidence. Nor can we put before the orthodoxy of political science a bundle of musty papers detailing the agreement whereby any one of the thousands of common law corporations called a state come into existence. We know only that states, huge masses of fabricated will-power, expressing themselves through an organization of one

form or another, exist and have existed as far back as history's dull nose holds the scent.

A science is an estimate: All substance is composed of earth, air, fire or water. That is not true; but it was true—as far as any science is ever true. It was the most reasonable explanation of things at the time of its currency. All matter is composed of indivisible, infinitesimal, ultimate particles called atoms. This is not true, but it was true—as far as any science is ever true. We of the present generation, sacrificed on that altar, burned our candles and mumbled the ritual. Again, Newton's law of gravitation is not true: It is practicably true, but not perfectly true. (At least, that is what Einstein tells us.) All knowledge, all scientific law is merely the current estimate of things and their workings—a temporary working agreement among the experts. Political science needs a new estimate, a new working agreement as to the nature of the state. Man is a political animal, hence the state—that estimate of the state is as old-fashioned as an earth-air-fire-and-water theory of matter.

Imagine a populated area where no political arrangements exist; then compare this with a known example of a primitive tribal state; the net difference is the concept or idea, the state. Is it not clear that this difference could come into our original situation only through the formation (implicit if not explicit) of a common law corporation, an artificial, imaginary entity imbued with power expressed through organization? This organization is government. The donated, isolated mass of power is the state. It is human power. No additional human power could come from without. It must then have been fabricated from fragments of free will surrendered by all included individuals. Each individual (we are speaking figuratively now, Mr. Bryan) whittles off a portion of his free will, his right to act out his mind. He pools this with similar bits of free will surrendered by all whom inclination or circumstance force into his group. The new, artificial, pooled will, an externally irresponsible, planetesimal mass of will-power, is the state.

It is probably true that the protozoan state was the family, that it was sinewed by the necessities of regeneration and subsistence, that organization or government came in only as a tertiary consideration to further these fundamental ends; but these facts do not forbid our considering, in a purely political study, that we are dealing with a purely political growth. The advantages of organization, vitalized by delegated power, would have sufficed to bring forth such units without the biologic-economic bond. That tertiary political sinews grew within an already living body ought not to obscure the fact that they could have formed a body of their own. On this warrant do we postulate the fabrication of an imaginary person, a common law corporation, *the state*, exercising certain specific, delegated functions through its agent or system of agents, *the government*, according to formulated rule and accumulating custom, *the law*.

Let us be more specific: Here is a primordial settlement in a luxuriant natural basin. From hill top to hill top, the basin is twenty miles across; it supports a population of a hundred persons. Every one is free, totally and dismally free. Free as the fox under the hill, free as the wolf, the grasshopper or the butterfly. There is no law, no government, no state. There are a number of semi-observed courtesies and conventions not unlike our present international "law." But when passions rise and reason falls a prey to ambition these courtesies and conventions are forgotten.

One canny and far-seeing soul gathers a hoard of succulent roots, stores them in a cave for the winter. Two covetous

beasts of fellows plot, kill him and take the roots. Every one hears of it. It is deplored. The killing of a man is declared bad form. How can any one collect roots or otherwise provide for life, if it is but to court a violent end? Again and again this killing nuisance breaks out. Free expression of every impulse is manifestly bad. The exercise of free will in the taking of life or property must be surrendered by all individuals. It must be pooled: trusteed. A conclave is held. The right to kill and the right to take are placed in trust. Many object to this rank interference with personal liberty. But they are taken outside and their heads are cracked—a fitting initial exercise, by the trustees, of the newly created, pooled mass of power.

The trustees may be the whole people. That would be a democracy, a town-meeting government. Or a group of representatives may exercise the power—a republican form of government. Or there may be but one trustee (L'etat, c'est moi), a great, burly, hoary fellow, strangely but pleasantly honest, even and reserved. Probably the single chieftain ruling alone was the earliest form of government, and all primordial states were family or clan matters.

We admit there is no scrap of historical evidence of such a meeting of minds, and we let the truth of our theory rest solely upon the fact that it explains—offers the best *working explanation* of the state. The most elderly and respected of theories in whatever science has no higher claim to place.

Can zoologists prove an agreement among sea lions that flippers front and nothing rear were better than four legs —or furnish documented description of the drawing in of pedal extremities and the webbing over of digits? And perhaps their explanation does not follow the pattern of perfect truth. All turning-points in science shout denial. Perfect truth is as indefinite as the ether, but quite definitely, the history of science is a graveyard of things less perfect. But here is the present working agreement among specialists in zoology. Within their experience, it works, it explains. From the long range point of view it is only workably true; but for the moment that is now passing since nothing better offers, it is, in merely scientific sense, absolutely true. The earth-air-fire-and-water theory of matter, the atomic theory of matter, the proton-electron theory of matter, all true in their day—true as long as they offer the best available *working explanation* of things—are superseded when something new wins the confidence of sane minds.

Man is a political animal, hence the state—this theory of the state was true from Aristotle to Gettell. Lazy, presumptuous and very thin, still it formed a working agreement. But a common-law-corporation theory of the state throws far more light upon the essential nature and workings of the state. It is a *working explanation*. For to-day, true —true because it polarizes, arranges about itself, the facts of political life more neatly than any current explanation.

OYSTER FARMING[1]

By HERBERT F. PRYTHERCH

U. S. BUREAU OF FISHERIES

As you partake of a piping hot oyster stew or six cool delicious bluepoints on the half shell, it may interest you to know how this shellfish has been produced.

The oyster industry of the United States constitutes its most valuable fishery, yielding annually about 73,000 tons of food, employing over 65,000 persons, and producing each year a crop valued at over $14,000,000 as it is taken from the water. The oyster fishery is conducted in every seacoast state from Cape Cod to the Rio Grande and from Puget Sound to San Francisco.

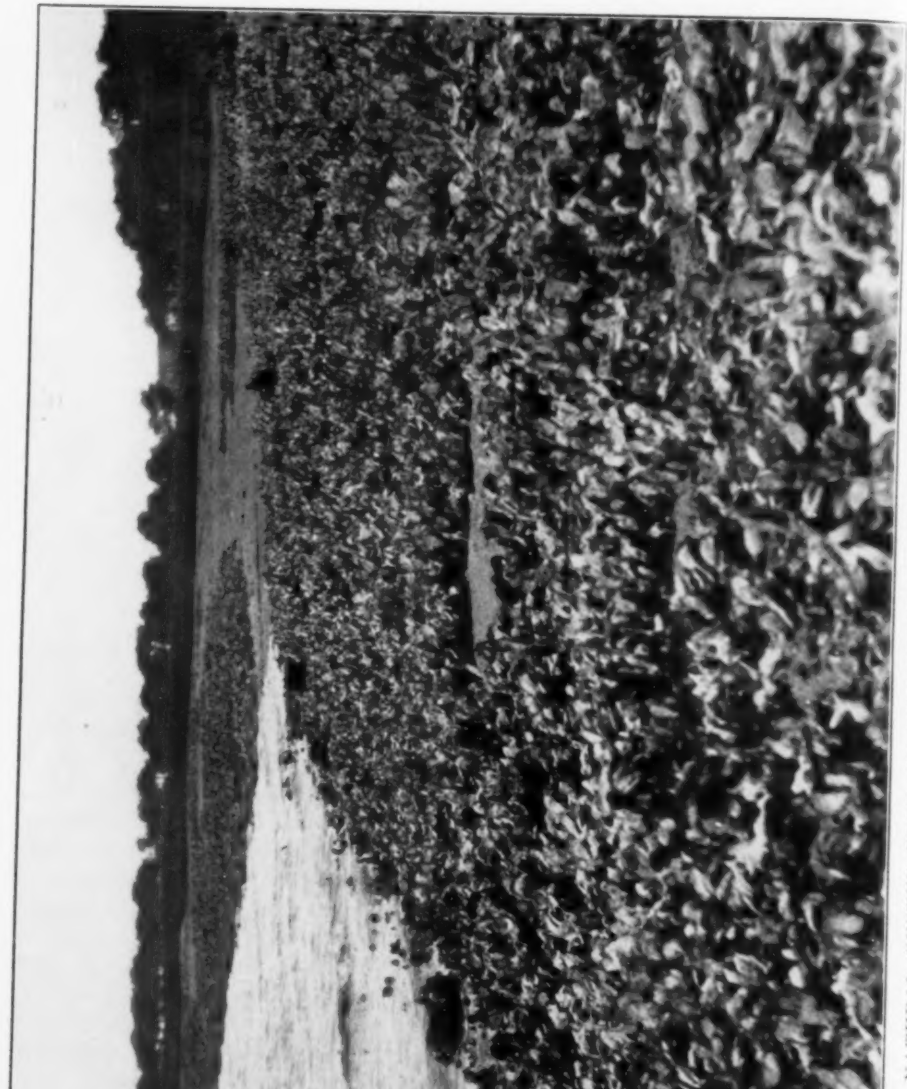
Oysters do not grow in the open sea, but in harbors, bays, river mouths, or in other partially enclosed bodies of water which are made brackish by the drainage of fresh water from the land. In such places, as for instance, Long Island Sound and Chesapeake Bay, the mixture of fresh and salt water furnishes the conditions which are necessary and favorable for oyster growth and propagation. The oysters that are produced in the United States come from two main sources, namely, the natural beds and the cultivated beds.

The natural oyster beds, covering millions of acres of bottom in our coastal waters, represent a great national resource, but like most of our natural resources they have become sadly depleted. When the first settlers came to the shores of America, one of the most impressive indications of the richness of the new land was the great abundance, the large size and the excellence of the oysters which they found. The Indians along the coast subsisted largely on oysters, leaving great mounds of shells as mute evidence of the fact that they celebrated their feasts with this delicious bivalve. With the advance of civilization and increased population, the demand for oysters became greater and resulted in intensive operation of this fishery. In a very short time the supposedly inexhaustible natural beds were considerably depleted, so it was obvious that, lest the supply should fail, man must lend a hand much as he had in the raising of land crops.

The oyster lends itself readily to cultivation, first, because it is unable to move of its own volition from the beds on which it is placed; second, because it can withstand rough handling and long exposure to air; and third, because of its interesting and unusual life history, which makes possible unique methods for controlling and increasing its production.

Oysters spawn during the summer months, a single female oyster producing from ten to sixty million eggs, which are forcibly discharged into the water and after fertilization by the elements from the male oyster develop into what are known as oyster larvae. These larvae, which are microscopic in size, swim about in the water or lie on the bottom for a period of about two weeks, after which they cement themselves to some clean hard surface such as that of shells or rocks. These tiny oysters when attached or "set" on an old shell appear as small dark specks, and some idea as to their size can be gained from the fact that over a thousand have been found on a single square inch of shell,

[1] One of the Smithsonian series of radio talks arranged by Mr. Austin H. Clark and given from Station WRC, Washington.



A NATURAL OYSTER REEF ON THE COAST OF SOUTH CAROLINA

A NATURAL OYSTER REEF ON THE COAST OF TEXAS

with very few touching each other. In oyster culture and oyster farming this interesting attachment or setting period in the life of the oyster is of prime importance. Man has taken advantage of this habit by placing old shells and other suitable objects in the water to which the oyster larvae readily attach themselves, and thereby he is able to collect and save vast numbers that would otherwise be lost. By placing shells on firm bottoms near the adult oysters or the natural beds he is able on the grounds he has prepared to obtain a considerable crop of seed oysters. From the first experiments made with the planting of shells in East River, N. Y., in 1855, the practice of oyster culture has grown steadily until at the present time over half of the oysters produced in this country come from privately owned and operated artificial beds. Beginning in the shoal waters along shore, the oyster growers have extended their operations into the deep open waters of Long Island Sound and Chesapeake Bay, converting thousands of acres of useless bottom into valuable food-producing areas. The modern oyster farmer, after acquiring many acres of suitable submerged bottom, conducts his operations along the following lines:

The grounds are carefully cleaned by dredging from them old shells, débris and natural enemies of the oyster, such as starfish, conchs and drills. On part of the grounds adult oysters are planted for growing purposes and also to serve as a spawning bed. In the early summer just previous to the time of spawning, thousands of bushels of old oyster shells from the shucking houses are planted on the grounds in the vicinity of the spawning beds; usually from five

PORTION OF AN OYSTER REEF ON THE COAST OF GEORGIA

hundred to one thousand bushels are used per acre. After the shell planting is completed the beds are left undisturbed for the remainder of the summer, the oysterman spending most of his time overhauling his boats and equipment. In the early fall an inspection of the shells is made to determine how heavy a crop of seed oysters has become attached to them.

If there is no danger of this new crop being buried or washed ashore by the fall and winter storms, they are left there until spring, at which time they are transplanted to the growing grounds. The growing grounds are generally located in deeper water and are known to be areas unfavorable for oyster reproduction so that oysters placed there are not covered and overcrowded by successive generations. On these grounds the seed oysters are given ample room for growth and reach marketable size in from two to five years, according to the locality in which they are grown.

The oyster farmer from years of experience has learned that not only are some areas favorable for collecting seed and others for growing the oysters, but that still others are excellent for fattening them. From six months to two years previous to the time of marketing, the oysters are placed on these fattening grounds, where with clean water and an abundance of food they become fat, tender and rich in vitamins and mineral salts.

The oyster in feeding opens its shell, creates a current of water through the gills, and filters from the water passing

through thousands of minute plants called diatoms, which are its principal food.

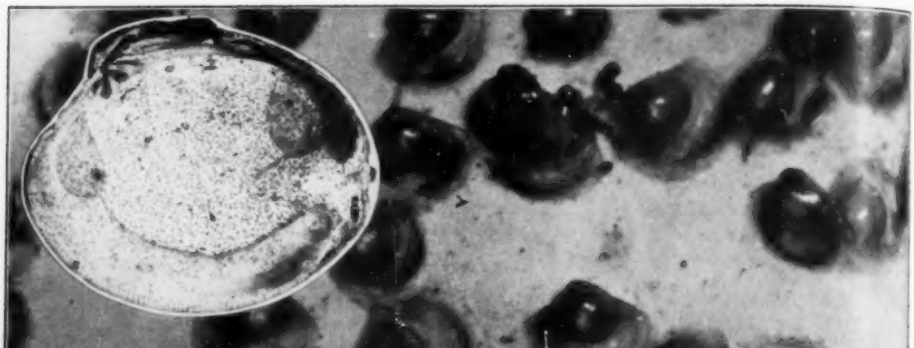
By means of a new apparatus invented in the laboratory of the Bureau of Fisheries, it has been possible to measure accurately the amount of water which the oyster drinks. Feeding for about twenty hours a day, the oyster uses approximately three quarts of water per hour at a temperature of 70°, or, in other words, fifteen gallons per day, from which it extracts more than 99 per cent. of the suspended material and food particles.

To show the magnitude of this process, let us make the following comparison: The District of Columbia uses sixty-five million gallons of water daily, yet this amount would be consumed in the same period of time by the population of an oyster bed only one tenth the size of the White House grounds. Many of our natural oyster beds which are as large as the tidal basin consume daily three thousand times as much water as is used by the entire District of Columbia.

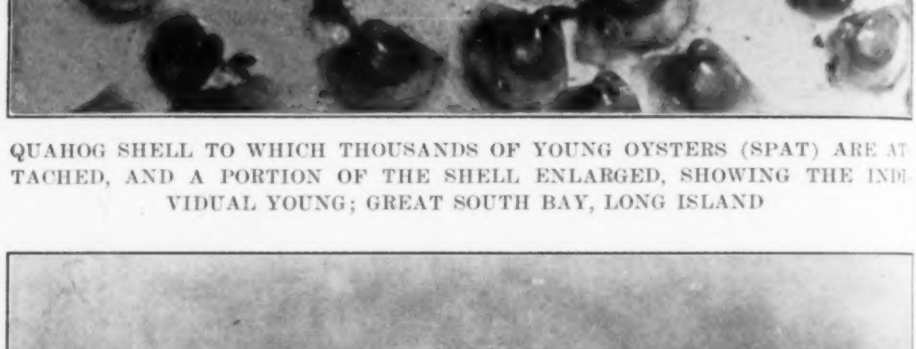
As the temperature of the water becomes lower in the fall and winter the feeding activities of the oyster become less, until at a temperature below 44° it ceases to take in water and goes into a state of inactivity or hibernation. This period of hibernation lasts until the water warms up again in the spring, when the oyster resumes feeding.

The oysterman, in gathering his crop in the cooler weather from his selected maturing beds, brings the oysters to market when they are in the best possible condition. The chief method employed in taking the oysters from the beds is the use of a dredge which is dragged over the bottom. Each gasoline- or steam-driven boat usually operates from two to four dredges, with which it is capable of gathering from one to five thousand bushels of oysters per day.

OYSTERS SPAWNING NATURALLY ON A TRAY IN A LARGE SPAWNING TANK



QUAHOG SHELL TO WHICH THOUSANDS OF YOUNG OYSTERS (SPAT) ARE ATTACHED, AND A PORTION OF THE SHELL ENLARGED, SHOWING THE INDIVIDUAL YOUNG; GREAT SOUTH BAY, LONG ISLAND



A BOAT LOAD OF BRUSH PREPARED FOR THE CATCHING OF OYSTER SPAT; GREAT SOUTH BAY, LONG ISLAND

Between the shell-planting and the harvest, an interval of from two to five years, the oyster farmer assumes many hazards. In northern waters, with unfavorable weather conditions, he often fails to obtain a crop of oysters on the material he had planted; while in the South Atlantic and Gulf waters the crop is often so heavy that the oysters are overcrowded, poorly shaped, and suffer from lack of food. They are never safe from their natural enemies, one of the worst of which is the common starfish, which wraps its arms about the oyster, pulls the shells apart and, by turning its stomach inside-out, absorbs the oyster as it lies within the shell. The planter fights the starfish by dragging over the beds large mops of rope yarn. The starfish become entangled in the threads and are drawn up and killed by plunging the mops into vats of boiling water placed on deck.

The drill, or borer, a little marine snail, is another destructive enemy, which, using its tongue like a rasp, bores a hole through the shell and licks the delicious meat within. In southern waters schools of drumfish invade the beds and feed on the oysters by grinding them to fragments between their powerful teeth. Flood waters from the land and storms rolling in from the sea take a heavy toll, either killing the oysters by subjecting them to fresh water for prolonged periods, or burying them in the bottom by powerful wave action.

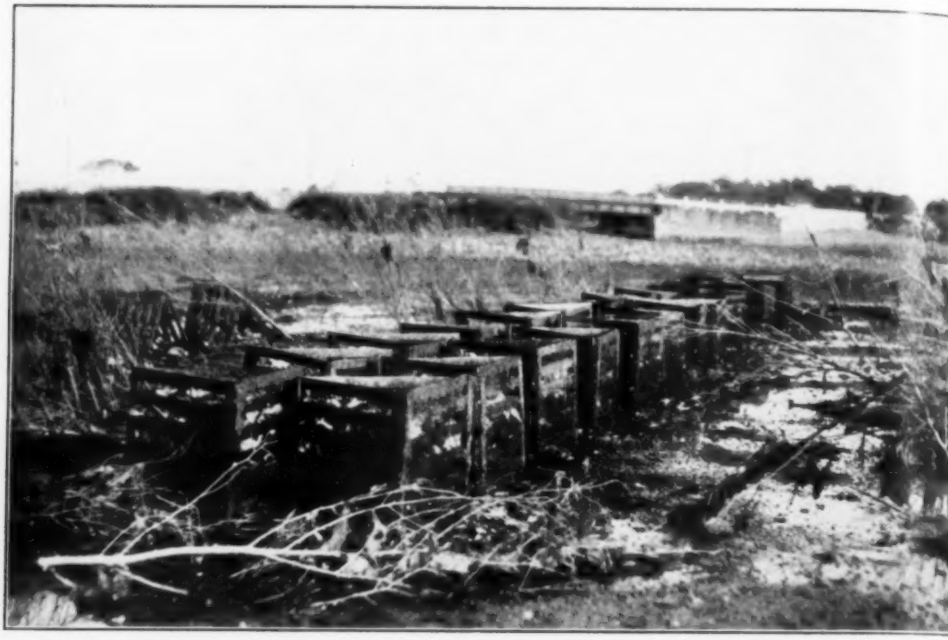
In spite of the extensive development of oyster culture, these and various other factors have brought about a constant depletion of the oyster beds, both natural and cultivated, resulting in an alarming decline in the productiveness of our great oyster fisheries.

In order to determine new methods for maintaining and increasing the supply of oysters, extensive studies and experiments have been made by the Bureau of Fisheries in Massachusetts, Connecticut

OYSTERS ON A TREE; YOUNG OYSTERS ATTACHED TO A BIT OF BRUSH; SOUTH CAROLINA

and various other coastal states. Two methods have been developed for increasing the production of seed oysters, since the decrease of these has been one of the principal causes of the decline of the industry.

One method consists in the use of brush for collecting the seed oysters on the tidal flats, or, in other words, the growing of oysters on trees. A short time before the oysters spawn branches from four to eight feet long are forced into the bottom and arranged in conical stacks. In a week or two the oyster larvae become attached to them, thousands covering each branch. The following spring the branches are either transplanted to growing grounds, or the oysters are detached from them and planted singly. There are many advantages in using brush, chief of which are that it utilizes soft mud bottoms and that in a

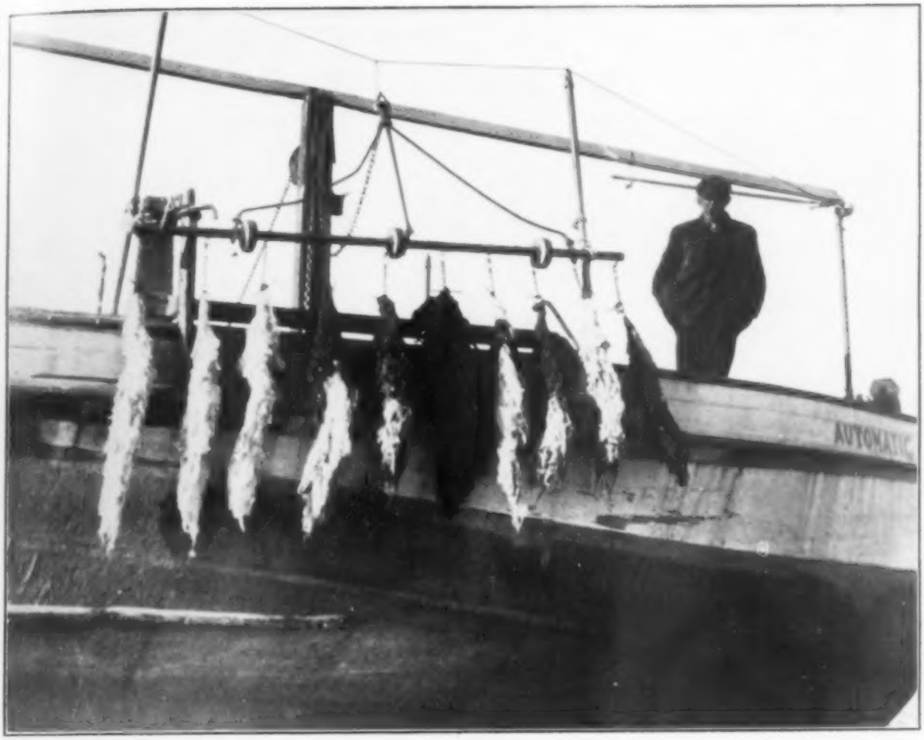


CRATES FILLED WITH OYSTER SHELLS, PREPARED FOR THE COLLECTION OF YOUNG OYSTERS, OR SPAT

year it will disintegrate or be destroyed by shipworms, so that the seed oysters attached to it break apart as single individuals.

The idea of using brush is not new, as it was first used by the Romans about the time of Julius Caesar, and the method is still practiced on the coast of Italy and in other parts of Europe. Also in Australia, the tidal flats are covered with mangrove sticks, while in Japan the grounds appear like forests of bamboo heavily laden with oysters. The birch and oak brush plantings which were made in Milford, Connecticut, and in Georgia, were successful, and clearly indicate that in the United States, especially in southern waters, brush can be used on a practical commercial scale and will give excellent results.

During the past summer an entirely new method was developed for the control and production of seed oysters in northern waters. It consists essentially in the establishment of spawning beds in bays, harbors and river mouths, and the planting nearby of crates filled with shells for collecting the oyster seed. The crates were constructed of spruce lath, were triangular in shape and held two bushels of shells. In Milford Harbor, Connecticut, and Wareham River, Massachusetts, four hundred crates were set out and these collected over five million seed oysters. The advantages of using the crates are: that eight or ten times more seed oysters can be produced on a given area than by the ordinary methods of shell planting and that they can be placed on barren mud flats and sand bottoms or directly over the spawning



A STARFISH MOP, USED FOR THE REMOVAL OF STARFISHES FROM OYSTER BEDS; MILFORD, CONNECTICUT

beds, thereby obtaining the maximum use of the limited inshore areas.

Further studies of the oyster, its life history and environment are being made so that the oyster farmer, like the agriculturist, can control increase and protect his crop by the application of scientific methods.

WATCHMAKERS AND INVENTORS[1]

By CARL W. MITMAN

U. S. NATIONAL MUSEUM

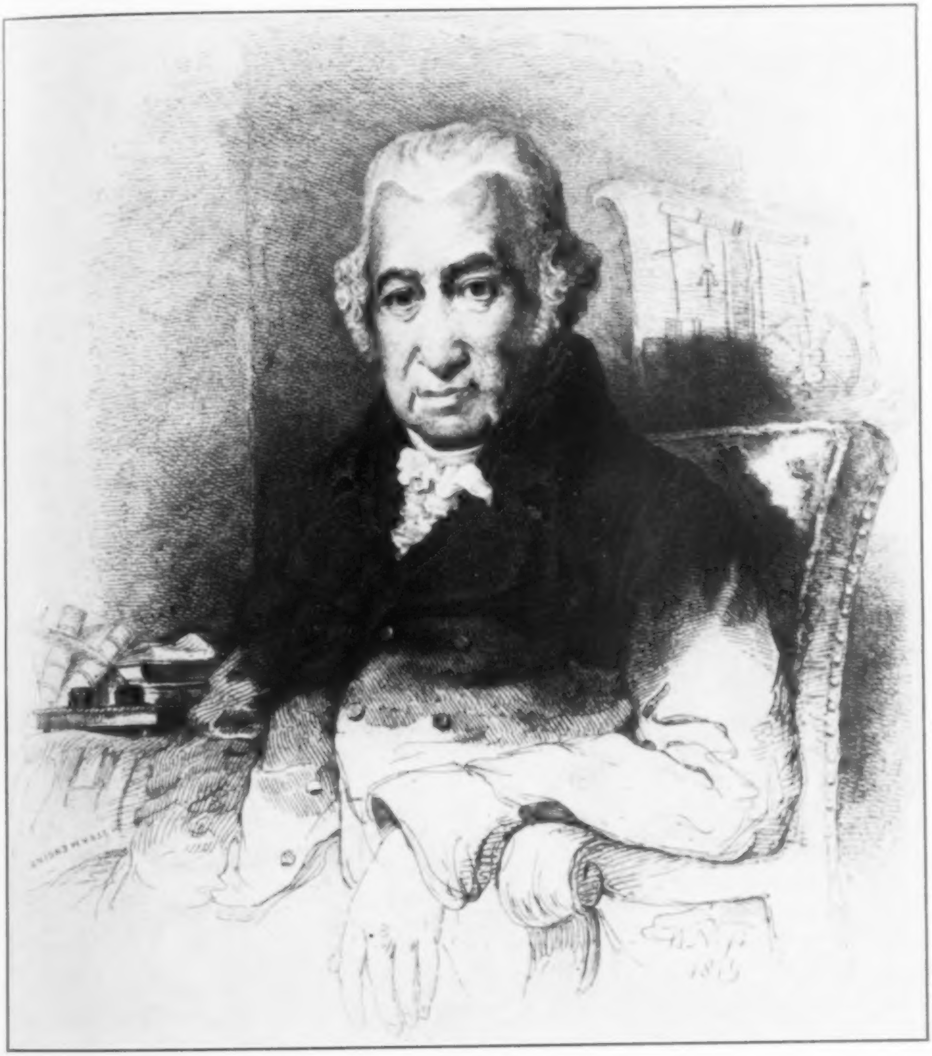
In one of our modern encyclopedias appears the statement that more basic inventions, except those in electricity and industrial chemistry, are the results of efforts of watch and clock makers than of any other professional group. It is a statement that I can hardly doubt, for I could consume much time by giving simply an alphabetical and chronological list of the names of inventors whose basic mechanical experience was that of watchmaking.

We are all pretty generally agreed that the world to-day is a rather pleasant place to live in, particularly the United States, and were we to make a search for the causes back of our present standards of living we would eventually find three basic ones; first, our wealth of natural resources; second, our intensive application of mechanical power and tools in the exploitation of these resources; and third, the thorough development of our transportation systems for the distribution of commodities. Our resources were placed here during the formation of the earth and simply awaited discovery by our ancestors, but it was James Watt by his invention of the separate condenser who made possible the development of the true steam engine, which, in turn, brought about what is sometimes called "the industrial revolution," from which time mechanical power and mechanical contrivances gradually replaced hand power and hand appliances.

Whether the story of Watt and the teakettle is authentic or not is immaterial. We know that his father's business of selling ship's supplies in Glasgow, Scotland, did not appeal to young Watt, and he went to London and paid the sum of \$500 a year to a master of the Watchmakers' Guild for instruction in the watchmaker's art. After an apprenticeship lasting two years he had advanced on his own ability to such an extent that he set up shop in Glasgow as an instrument maker. His chief work was that of repairing and constructing the scientific instruments used at the University of Glasgow, and on one occasion a working model of a Newcomen atmospheric engine was sent to him for repairs. This was the first steam engine that Watt had ever seen, and it was while working on it that he made his discovery that by the use of a separate vessel for condensing the steam greater efficiency and more work could be obtained from the Newcomen engine.

As we look back to-day, we can see that the most direct effect of Watt's invention was the gradual development of machines to replace hand labor, giving employment to many where formerly there were openings for a few, with the result that in a comparatively short time the production of industrial commodities increased. This increase, in turn, brought to light the shortcomings and the inadequacy of the prevailing means of transportation. A few fearless men in England boldly suggested the use of steam engines on wheels as a solution to the problem and proceeded to build engines to prove their claim. Richard Trevithick was one of these individuals, but the locomotives that he built were unable to stand the test as to their economic value.

[1] One of the Smithsonian series of radio talks arranged by Mr. Austin H. Clark and given from Station WRC, Washington.



JAMES WATT

INVENTOR OF SEPARATE STEAM CONDENSER IN 1769.

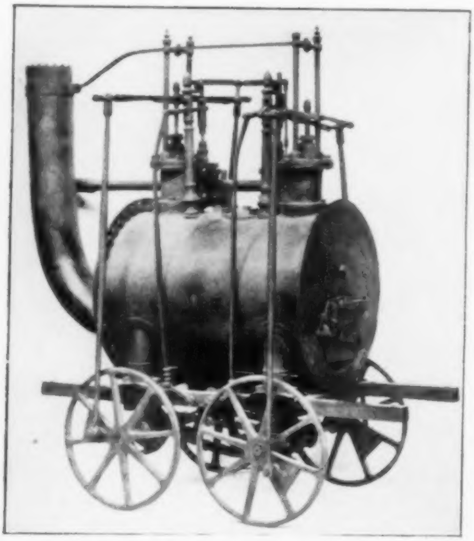
From a painting by Clyde O. DeLand

THE TRIAL OF JOHN FITCH'S STEAMBOAT ON THE DELAWARE RIVER AT PHILADELPHIA IN 1786

Another Englishman who believed as did Trevithick was George Stephenson, and it was he who constructed a locomotive which definitely proved the superiority of the steam engine over the horse and brought about the beginning of the "iron horse era." Stephenson received his early training about the coal mines in the north of England, first as a pumping engine man and later as an engineer. His wages were very low; he appreciated the value of an education which he did not possess; and he was most anxious that his only son should be educated. He secured whatever extra work and money he could, which came mainly from the repair of the timepieces of the townspeople in the several towns in which he lived. It was through the experience which he thus gained, combined with his experiences with the Newcomen and Watt pumping engines, that he was enabled to develop and construct in 1825 the locomotive "Locomotion," the first practical steam locomotive in the world.

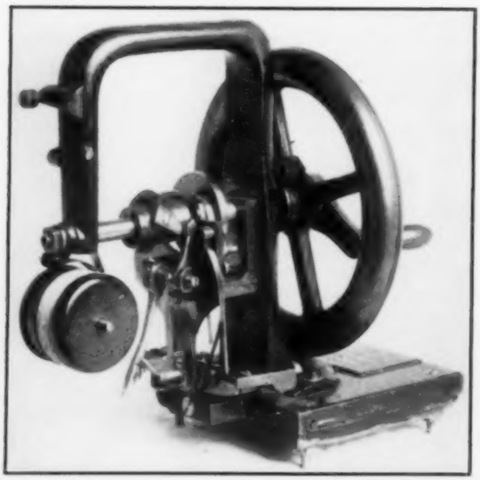
Several years ago I came across a little squib which illustrates rather well the relationship which now exists between the manufacturing and transportation industries. It went something like this: "My son, the chickens we eat all come from little eggs." "That's funny," said the son, "I always thought it was just the reverse." In other words, the manufacturer to-day must have a transportation service and the transportation industry must have goods to carry. Both of these industries received their impetus from the discoveries and work of Watt and Stephenson, whose chief training was that of watchmaking, and both have advanced and developed materially through the discoveries and efforts of men of like training.

It is generally conceded that many new discoveries are the product of several brains, and while this may be true in our day of almost instantaneous communication, it can hardly be said of the fifty-year period following the signing of the Declaration of Independence. Then it was physically possible for neighbors, as it were, to be working on and developing the same idea without any knowledge, one of the other. Knowing what we do to-day, we are prone to be rather hard on our forefathers, who at the beginning of the nineteenth century looked with scorn and derision upon the men who advanced what the majority thought were impossible ideas, but I doubt whether much of this skepticism of new ideas has entirely disappeared even today; in proof of which let me ask what you who were twenty-one or over in 1900 thought of the men who were interested in flying machines. It was skepticism such as this that undoubtedly robbed the United States of the credit of the invention of the locomotive, for, as early as 1786, Oliver Evans, of Philadelphia, asked the legislature of Pennsylvania, and later that of Maryland, for the sole right to operate carriages propelled by steam on the highways. Separated as he was by thousands of miles of water from England, it is not hard to believe that his work was original with him and not an improvement on something that had been developed elsewhere. The Pennsylvania legislature refused Evans' request, but the Maryland legislature granted it on the ground that no harm could be done anybody. But with this monopolistic grant no capital was obtained, even after he had built a four-horsepower steamboat in 1804, put wheels under it, and ran it through the streets of Philadelphia as proof of the soundness of his ideas, and had gone so far as to bet $3,000 that he could build a steam wagon to run on a level road against the fastest horse any one could produce. His entreaties fell on deaf ears, and it required the importation of a locomotive from England twenty-five years later to convince a very few that the steam locomotive not only could move without being



ORIGINAL MODEL OF A LOCOMOTIVE

CREDITED TO OLIVER EVANS, OF PHILADELPHIA, IN 1804.



ELIAS HOWE'S SEWING MACHINE, 1846

pulled by a horse but that it could transport merchandise more cheaply than the horse.

The constant hammering which the public generally in our own country received from such men as Evans, Dearborn, Stevens and Cooper brought about gradually the acceptance of the idea that the railway was the best means of obtaining better transportation. But the people were not convinced that the steam locomotive should be the power used. Several of the railroads, chiefly the Baltimore & Ohio and the South Carolina, had already received their charters and were planning to install horse-drawn carriages, when, as a result of the successful trials on the Baltimore & Ohio tracks of an experimental locomotive, built by Peter Cooper in 1829, he being financially able to prove his assertions as to the steam locomotive, the Baltimore & Ohio changed its plans and offered a prize of $4,000 for a steam locomotive capable of pulling fifteen tons at the rate of fifteen miles an hour. In due time the company received five locomotives, two of which were made by watchmakers in Philadelphia, namely, Stacey Costell and Ezekial Childs.

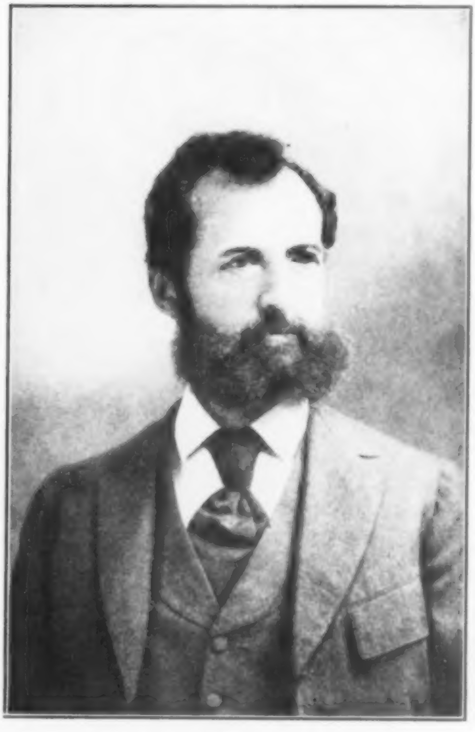
The radical policy of the Baltimore & Ohio Railroad in ordering a steam locomotive, coupled with the fact that the Delaware & Hudson Company had ordered four locomotives from England, aroused more and more public interest in the steam locomotive, and in 1829 the Philadelphia Museum endeavored to satisfy this interest by exhibiting a working model of a locomotive. The museum secured the services of Matthias W. Baldwin to build it. Baldwin was a watchmaker and just prior to this time had enlarged his establishment to manufacture bookbinding tools and machinery for calico printing. He had also just completed a miniature steam engine. The model locomotive he built for the museum drew two miniature coaches on a circular track. As a result of this work and the skill which he showed in the construction of the model, the officials of the newly organized Philadelphia, Germantown & Norristown Railroad Company sought Mr. Baldwin's

MATTHIAS W. BALDWIN
FOUNDER OF THE BALDWIN LOCOMOTIVE WORKS AT PHILADELPHIA IN 1832.

services in the construction of a full-sized locomotive for their railroad. He accepted and built "Old Ironsides," which was tried out in the latter part of 1832, and its trial marked the first movement by steam on a railroad in the state of Pennsylvania. Baldwin thereafter continued building locomotives and organized the well-known Baldwin Locomotive Works.

About the time Oliver Evans was pleading for capital to build his steam-propelled road vehicles, John Fitch was pleading for capital to build a steamboat. When quite young he left home and apprenticed himself to a Connecticut watchmaker. This man, however, was of the type who believed that to teach an apprentice anything would decrease the amount of work accruing to himself, and accordingly he kept his tools under lock and key and did his repair work as far away from Fitch as possible. It was only by stealing the tools when the master was away and by slyly watching him work that Fitch acquired all his knowledge of mechanics. Apparently it was sufficient, however, for a few years later in 1786, through his own efforts, he accumulated sufficient money to build a steamboat which made regular trips on the Delaware River between Philadelphia and Trenton.

The establishment of the new industrial order early in the nineteenth century brought with it vast opportunities for the application of inventive talents and the watch and clock makers are well represented in the real inventions which were made during this century. It was Joseph Rogers Brown, a New England clockmaker, a number of whose tower clocks are still in use after seventy-five



OTTMAR MERGENTHALER
INVENTOR OF LINOTYPE, 1883.

years, who in 1850 invented the first automatic machine in the United States for graduating rules and which is still in use, meeting all the requirements of modern accuracy. He too, in 1852, introduced the vernier caliper reading to a thousandth of an inch and built the first universal grinding machine. While apprenticed to a Boston watch and instrument maker, Elias Howe overheard a customer remark that a fortune awaited the man who could invent a sewing machine, and four years later in 1846 he received his sewing machine patent.

The reams of printed matter available to us to-day, whether books, magazines or newspapers, were made possible by a citizen of the United States, Ottmar Mergenthaler, who emigrated from Germany in 1872 with nothing but his completed apprenticeship as an expert watchmaker and thirty dollars in cash. Twelve years later he successfully demonstrated his new idea of setting type mechanically and subsequently offered to the world the linotype.

I believe you will agree that the brain of the watchmaker, as a class, is wonderfully versatile. As a further proof of this, let me close with the epitaph which a watchmaker composed for himself more than a hundred years ago. Some one else, of course, filled in the date of his death.

Here lies in a horizontal position the outside case of
George Routledge, Watchmaker.
Integrity was the mainspring and prudence the regulator of all the actions of his life;
humane, generous, and liberal,
His hand never stopped till he had relieved distress.
So nicely regulated were his movements that he never went wrong, except when set going by people who did not know his key.
Even then he was easily set right again.
He had the art of disposing of his time so well, that his hours glided away, his pulse stopped beating,
He ran down November 14, 1801, aged 57,
In hopes of being taken in hand by his Maker,
Thoroughly cleaned, repaired, wound up, and set going in the world to come, when
Time shall be no more.

TYNDALL'S EXPERIMENTS ON MAGNE-CRYSTALLIC ACTION[1]

By Sir WILLIAM BRAGG, F.R.S.

In 1845, Faraday made the surprising discovery that the vast majority of substances, not merely iron, nickel and cobalt, are affected by a magnet; and showed also that the action is repulsive quite as often as attractive. Faraday's results excited the greatest interest and were the starting-point for many other researches. In fact, they paved the way for the work of Thomson and Maxwell, who came thereby to the establishment of the laws of electromagnetism. Among the many workers who followed Faraday was Tyndall, who made certain interesting discoveries relating to the behavior of crystals in the magnetic field.

A very lively discussion sprang up as to the mode of interpretation of the new discoveries, particularly that of the so-called diamagnetism. On one hand, Faraday was satisfied that he could describe them in terms of his "lines of force": the majority, including Tyndall, referred everything to the existence of poles, magnetic and diamagnetic. Tyndall's experimental work, and the consequences which he drew from it, were devoted to the support of these views. When Faraday's conceptions prevailed it became clear that Tyndall's interpretation of his own experiments must have been incorrect. His collected account of his researches, published in the well-known "Diamagnetism and Magne-crystallic Action," never became a link in the chain of argument.

The recent analysis of crystal structure by means of X-rays throws some new light on those experiments of seventy years ago. We can see more clearly where Tyndall's conclusions were in error. But at the same time the experiments of Tyndall are seen to be closely related to a modern research of immense importance, that of the effect of stress on the constitution and physical properties of materials.

Faraday's First Observation of Diamagnetism

On September 13, 1845, Faraday made one of his most important discoveries, that of a relation between magnetism and light. He found that when plane polarized light was made to traverse a piece of his "heavy glass," a borosilicate of lead, in a direction coinciding with that of lines of magnetic force, the plane of polarization was rotated. He had thus been successful in showing that the action of a magnet did not require the cooperation of a magnetic substance such as iron for its manifestation, but might be directly connected with a substance of a different kind, namely, glass, and a different activity, namely, light. In the following months he tried to find some other connection between magnetism and this glass. He floated his glass on a liquid and tried whether he could move it by a magnet, without result. But on November 4 he succeeded in his search.

> The bar of heavy glass, 1 6/8 of an inch long and . . . [2] of an inch square, was suspended by cocoon silk in a glass jar in principle as before and placed between the poles of the last magnet. When it was arranged and had come to rest I found I *could* affect it by the magnetic forces and give it position. Thus touching diamagnetics by magnetic curves and observing a property quite independent of light by which we may probably trace these forces into opaque and other bodies as the metals, etc.
>
> If 1 was the natural position on making the poles magnetic the glass swung into position 2 and on to position 3. If contact was united after 2 the tendency to 3 was diminished, *i.e.*,

[1] Discourse delivered at the Royal Institution on Friday, January 21, and printed in *Nature*.

[2] Not filled in, but from other notes we know it was half an inch square.

was less than if there was no current. If whilst swinging contact of current was continued during vibration from 1 to 2 and broken from 2 to 3, then united from 3 to 2 and broken from 2 to 1 the bar was soon sent spinning round the whole circuit.

The word "diamagnetic" is here used to denote substances through which, on his views, magnetic lines were passing. It is not yet used as an antithesis to "paramagnetic." His new result obviously gave him intense pleasure, and in following it up he was so preoccupied that he did not even go to the meeting of the Royal Society on November 20, when his paper on the "Action of Magnets on Light" was read.

We can easily repeat the experiment, using a piece of the same glass taken from the store left by Faraday. It is not the actual piece, Number 174, as he tells us in his notes, for this can not be found. The glass turns slowly in the magnetic field, and its motions are obviously controlled by switching the current off and on. The glass tends to set itself *across* the lines of force running from pole to pole, not *along* the lines as a piece of iron would do; and obviously the effect is very small as compared with the violent movements of iron in the same circumstances.

The action may be described as a repulsion of the glass by the magnet; and sometimes the early workers on the subject constructed apparatus specially designed to show the repulsive effect more obviously, and to distinguish it from a mere turning action in a magnetic field, if indeed this could be done. We can illustrate this point, and at the same time the diamagnetism of bismuth, by using a piece of apparatus constructed by Tyndall.

Faraday's First Explanation of Diamagnetism

Faraday at first suggested that the diamagnetic effect was the antithesis of the ordinary magnetic effect. A piece of iron when placed between two poles became so magnetized that a south pole was developed upon it in that part which was nearest to the north pole of the inducing magnet and *vice versa*. Faraday's suggestion that the diamagnetic substance developed north and south poles where a magnetic substance would have developed south and north, respectively, was taken to be a satisfactory explanation. It was the constant endeavor of later experimenters to express their results in accordance with Faraday's hypothesis: even when development had reached a stage some distance ahead of that described in the original paper (*Phil. Trans.*, 1846, p. 21).

Faraday was himself the first to feel doubts as to the satisfactory nature of his explanation. His early results could conveniently be described as showing an exact antithesis between two classes; where one was attracted by a magnet, the other was repelled; where one set itself in a certain direction in the magnetic field, the other avoided that direction as much as possible. It seemed proper to describe them as being in exact antithesis to each other, and the word diamagnetism was adopted as a means of expressing the experimental result.

He prepared a list of substances which showed varying degrees of response to the action of the magnetic field, and the plan of the statement illustrates his first views ("Experimental Researches," Series XXI, No. 2424):

Iron	Alcohol
Nickel	Gold
Cobalt	Water
Manganese	Mercury
Palladium	Flint glass
Crown-glass	Tin
Platinum	Heavy glass
Osmium	Antimony
0° Air and vacuum	Phosphorus
Arsenic	Bismuth
Æther	

It is to be observed that those preceding air and vacuum are to be considered above zero or magnetic, those succeeding, below zero or diamagnetic, which is meant to imply a true antithesis.

In December, 1845 ("Experimental Researches," Series XXI, No. 2429), Faraday writes:

Theoretically, an explanation of the movements of the diamagnetic bodies, and all the dynamic phenomena consequent upon the actions of magnets on them, might be offered in the supposition that magnetic induction caused in them a contrary state to that which it produced in magnetic matter; *i.e.*, that if a particle of each kind of matter were placed in the magnetic field both would become magnetic, and each would have its axis parallel to the resultant of magnetic force passing through it; but the particle of magnetic matter would have its north and south poles opposite, or facing towards the contrary poles of the inducing magnet, whereas with the diamagnetic particles the reverse would be the case; and hence would result approximation in the one substance, recession in the other.

Even at that time, however, Faraday's views were not firmly established, and we may repeat an experiment of his which shows the nature of the contrary influences that were impressing him. A small glass tube filled with a weak solution of the magnetic substance iron sulphate sets itself axially between the magnetic poles; but if it is surrounded as it swings by a strong solution of the same substance, it sets equatorially. The tube appears to be magnetic as compared to air, but diamagnetic as compared to the strong solution.

Might not, on this analogy, all substances, and also air and vacuum, be magnetic, reacting to the magnet in the same way but to different degrees? And might not bismuth exhibit its peculiarities, not because it is in antithesis to iron, but merely because it is less magnetic than the air? Yet he writes as follows:

"Such a view also would make mere space magnetic, and precisely to the same degree as air and gases Now though it may very well be, that space, air and gases, have the same general relation to magnetic force, it seems to me a great additional assumption to suppose that they are all absolutely magnetic, and in the midst of a series of bodies, rather than to suppose that they are in a normal or zero state. For the present, therefore, I incline to the former view, and consequently to the opinion that diamagnetics have a specific action antithetically distinct from ordinary magnetic action, and have thus presented us with a magnetic property new to our knowledge" ("Experimental Researches," Series XXI. No. 2440, Dec. 1845).

The extract describes his first-formed opinion.

Plücker's Discovery of Magne-crystallic Action

The next important step is due to Plücker:

In 1847, Plücker had a magnet constructed of the same size and power as that described by Faraday, his object being to investigate the influence of the fibrous constitution of plants upon their magnetic deportment; while conducting these experiments he was induced to try whether crystalline structure exercised an influence (Tyndall, "Diamagnetism and Magnecrystallic Action," p. 2).

The first experiment made by Plücker gave an immediate and decided reply. The investigation of the behavior of several crystals led him to announce the following laws:

When any crystal whatever with an optic axis is brought between the poles of a magnet, the axis is repelled by each of the poles; and if the crystal possesses two axes, each of these is repelled with the same force by the two poles.

The force which causes this repulsion is independent of the magnetism or diamagnetism of the mass of the crystal; it decreases with the distance more slowly than the magnetic influence exerted by the poles.

There is some truth in Plücker's conclusions, but much correction is necessary. Tyndall pointed out in 1850 that they broke down completely when applied to calcium and iron carbonate. These two crystals are isomorphous; the former, Iceland spar, obeys Plücker's laws in that it sets its axis equatorially in the magnetic field, but iron carbonate sets its axis from pole to pole. Plücker had, however, done great service in directing attention to the peculiar behavior of crystals in the magnetic field.

In the autumn of 1848 Plücker was in London. Faraday writes in his laboratory notes:

16 Aug. 1848. Plücker has described to me certain of his results as to the crystalline diamagnetic relation and, as I understand it, the optic axis of a crystal having *one* optic axis tends to pass into the equatorial direction, or if a crystal have two optic axes then the line between them tends to pass into the equatorial direction.

25 Aug. 1848. To-day Plücker showed me for the first time some of his experiments.

First Optical Results

"A small rhomboid of Cal^e Spar was suspended by a single cocoon thread between my Elect. Magnet poles with the optic axis in a horizontal position. When the poles were very close as in the figure the diamagnetic force of the substance made it take the position shown in which the optic axis is axial to the magnet. But when the poles were opened out to distance of half or three quarters of an inch, then the mass pointed axially and the optic axis therefore equatorially. . . . There is a given distance between the Mag poles (pretty close) when a certain
or piece
rhomboid ∧ of Cal^e spar between them is so affected that the diamagnetic and the magneto-optic force is balanced, at smaller distances the piece points diamagnetic and at larger distances Magneto optic. So that on increasing the distance the magneto-optic force diminishes *less rapidly* than the magnetic force, and on diminishing the distance it increases less rapidly than the magnetic force. *But increasing or diminishing the strength of the magnet produces no alteration of this place of neutral action, it only increases or diminishes the strength of the action on each side of it: or rather the resultant of the two actions on each side of that neutral position.* So Plücker at least tells me, for I did not see that proved."

Plücker's experiment is readily shown; but a little care in adjustment is required. The dimensions of an equal-sided rhomb are rather too much the same in all directions: a somewhat more irregular piece is, I find, easier to work with. The effect is much more clearly seen with a good bismuth crystal, which was obtained in the following way. A little bismuth was melted in a glass tube which had been drawn to a point. The tube was placed in an electric furnace, from which it was made by clockwork to emerge very slowly. The fine end of the tube came out first, and the bismuth at the point was the first to solidify into crystalline form. The rest of the metal crystallized slowly as the emergence proceeded, and, in the circumstances, continued the structure and orientation of the first fragment. In this way, due to Bridgman, the mass contained large single crystals, not a mass of fine crystals as is usual when the solidification takes place rapidly.

When the crystal, which is ten times as long as it is broad, is placed in the magnetic field due to pointed poles, it sets strongly equatorially in accordance with the usual behavior of a diamagnetic body; but when the poles are withdrawn somewhat, it sets axially with equal strength.

The experiments of Plücker introduced a new effect which Faraday afterwards called "magne-crystallic action." It clearly deserves a name, since its manifestations added a complication to the diamagnetism which had already been observed.

The new discoveries presented so many forms when repeated with different crystals suspended in different ways and with different forms of magnetic field that the complications were not unravelled for some years. Some of the difficulties were due to the circumstances of the experiments and had no relation to the real question. One of these incidental effects was that of attractions and repulsions due to transient currents induced in bodies already suspended for observation between the magnetic poles when the magnets were excited. As is well known, the motion of a spinning block of copper is at once arrested by the action of such currents; on the other

hand, a sheet of copper held near a pole is sharply repelled when the current is turned off, and if properly suspended can be set into a rapid spinning. These effects had nothing to do with diamagnetism, but they were apparently the cause of confusion on some occasions.

Another great source of difficulty was the overpowering effect of iron impurities; the diamagnetic effects are so feeble in all cases that a mere trace of iron, nickel or cobalt is sufficient to mask them. So, for example, Plücker's experiments with antimony seem on this account to have been at variance with the true facts as proved by Faraday (Tyndall, p. 16).

Faraday's Researches on Magne-crystallic Action

In 1848, Faraday published a series of researches on the magne-crystallic phenomena, which cleared up some of the difficulties. But in 1850 he could still write as follows:

> Four years ago I suggested that all the phenomena presented by diamagnetic bodies, when subjected to the forces in the magnetic field, might be accounted for by assuming that they then possessed a polarity, the same in kind as, but the reverse in direction of, that acquired by iron, nickel, and ordinary magnetic bodies under the same circumstances. This view was received so favorably by Plücker, Reich, and others, and above all by W. Weber, that I had great hopes it would be confirmed; and though certain experiments of my own did not increase that hope, still my desire and expectation were in that direction. (2641) Whether bismuth, copper, phosphorus, etc., when in the magnetic field are polar or not is, however, an exceedingly important question; and very essential and great differences in the mode of action of these bodies under the one view or the other must be conceived to exist. I found that in every endeavor to proceed by induction of experiment from that which is known in this department of science to the unknown, so much uncertainty, hesitation and discomfort arose from the unsettled state of my mind on this point that I determined if possible to arrive at some experimental proof either one way or the other. This was the more important because of the conclusion in the affirmative which Weber had come to in his very philosophical paper. . . . (2642) It appeared to me that many of the results which had been supposed to indicate a polar condition were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of action; others, again, appeared to have their origin in induced currents.

Accordingly, he undertook a further series of researches which in the end brought him to regard all his effects as expressible in the simple form with which we are familiar. In his "Experimental Researches" he writes (Ser. XXVI, October, 1850, No. 2807):

> When a paramagnetic conductor, as for instance a sphere of oxygen, is introduced into such a magnetic field considered previously as free from matter, it will cause a concentration of the lines of force on and through it so that the space occupied by it transmits more magnetic power than before. If, on the other hand, a sphere of diamagnetic matter be placed in a similar field it will cause a divergence or opening out of the lines in the equatorial direction, and less magnetic power will be transmitted through the space it occupies than if it were away (see Fig. 1).

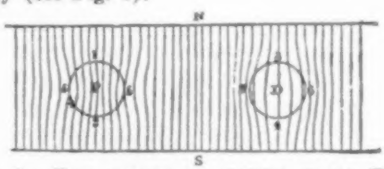


Fig. 1.—The figure is taken from Faraday's "Experimental Researches," and was drawn to show his conception of the passage of lines of magnetic force through paramagnetic and diamagnetic bodies respectively.

This describes diamagnetism generally: the complication of magne-crystallic action is described with equal simplicity:

> (2837) If the idea of conduction be applied to these magne-crystallic bodies it would seem to satisfy all that requires explanation in their special results. A magne-crystallic substance would then be one which in the crystallized state could conduct onwards, or permit the exertion of the magnetic force with more facility in one direction than another: and that direction would be the magne-crystallic axis. Hence, when in the magnetic field, the magne-crystallic axis would be urged into a position coincident with the magnetic axis by a force correspondent to that difference, just as if two bodies were taken, when the one with the greater conducting power displaces that which is weaker.

It is only a uniaxial crystal, of course, which possesses a single magne-crystallic axis; it is the axis of a certain spheroid. The facility of conduction in different directions in a biaxial crystal requires an ellipsoid for its representation.

This way of stating the rules allows us to see at once the principle of the experiments shown by Plücker to Faraday, which the latter so greatly extended. When the magnet poles were close, the crystal occupied a part of the field where the lines of force were very divergent. In such circumstances the orientation of the crystal would be determined by the general tendency for diamagnetic bodies to move from the stronger to the weaker parts of the field, and the crystal set its longer dimension perpendicular to the field; the optic axis was then parallel to the lines of force. But when the magnetic poles were separated the crystal covered a part of the field in which there was little divergence: the magne-crystallic action then took charge, and the crystal set itself so that the direction of worst conduction of the lines, *i.e.*, the optic axis, was at right angles to the lines.

Experimental Illustration of Magne-crystallic Action

A few simple experiments will serve as further illustration of these rules. We take a crystal of sulphate of iron which has the form of a thin plate: the flat sides are cleavage planes and the "conducting power" for Faraday's lines is far greater across the plate than along the large faces. In a uniform field the crystal plate sets equatorially therefore, and even when allowed to move up to one of the poles keeps its face normal to the lines. A thin plate of iron would stand on edge on the pole: but the magne-crystallic action of this paramagnetic crystal is exceedingly strong.

A bismuth crystal so suspended that its axis (it is a uniaxial crystal) is vertical has no magne-crystallic action. Its motions are governed by the general tendency of its mass to move from the stronger to the weaker parts of the field: in a uniform field it has no appreciable tendency to set itself in any particular direction. But when the crystal is hung so that the axis is horizontal, that axis tends strongly to set itself along the lines of force, as we saw before.

Naphthalene is a monoclinic crystal. Its magne-crystallic properties are represented by an ellipsoid, one axis of which coincides with the single axis of symmetry. The cleavage is very perfect and the crystals take the form of thin plates parallel to the cleavage planes. The axis is also parallel to the cleavage planes, and if the crystal is suspended so that this axis is horizontal and the cleavage planes vertical, the axis and the planes move into the equatorial position, in this way fulfilling symmetrical considerations. But when the crystal is hung in a uniform field, so that the axis is vertical, the cleavage planes set themselves at a certain angle to the field. One of the axes of the magnetic ellipsoid then lies along the lines, another across them: the third is vertical. There is no obvious relation between the cleavage plane and the first two axes. Finke (*Annalen der Physik* **31**: 149: 1910) has shown that this may be said of various crystals examined by him. If now the crystal be suspended from the other end of the axis, its cleavage planes will make the same angle with the field but on the opposite side of the medial line (Fig. 2). Faraday describes results of this kind which he obtained with a paramagnetic crystal of sulphate of iron ("Experimental Researches," Series XXI, Nos. 2634–7). Naphthalene is diamagnetic: like many other organic crystals, it shows the magne-crystallic effect very strongly.

These experiments will serve to show the great variety of effects that may be observed. All of them are, however, easily correlated by Faraday's concep-

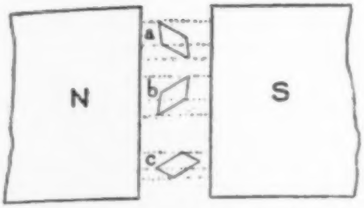


FIG. 2.—IN THIS FIGURE THE POSITIONS MARKED *a* AND *b* ARE POSITIONS OF EQUILIBRIUM OF THE NAPHTHALENE CRYSTAL IN THE MAGNETIC FIELD. THE AXIS OF SYMMETRY IS VERTICAL AND PERPENDICULAR TO THE PLANE OF THE PAPER. THE CLEAVAGE PLANE IS ALSO VERTICAL AND ITS INTERSECTION WITH THE PLANE OF THE PAPER IS THE LONGER SIDE OF THE RHOMBOID. THE FORM OF THE CRYSTAL SHOWN IN THE FIGURE IS NOT A NATURAL FORM, BECAUSE THE SHORTER SIDE OF THE RHOMBOID IS DRAWN PARALLEL TO ONE FACE OF THE CELL OF THE CRYSTAL LATTICE, WHICH FACE DOES NOT USUALLY OCCUR ON THE CRYSTAL. IT IS SO DRAWN IN ORDER TO SHOW THE RELATION BETWEEN MAGNETIC LINES AND THE LATTICE. IF THE CRYSTAL IS HUNG FROM ONE END OF THE *b* AXIS, THE POSITION *a* IS ASSUMED, AND IF FROM THE OTHER, THE POSITION *b*. THE POSITION *c* IS IMPOSSIBLE.

tion of lines of force. Let us remember that there are several variables and give due importance to each. The first of these we call diamagnetism, implying that the lines pass through the substance in question less easily than through the air or a vacuum. The second is called magne-crystallic action, in reference to the fact that in a crystal the lines pass more easily in one direction than another. A third variable is the crystal shape, which may also affect the set in the magnetic field when the latter is divergent. A fourth is the amount of divergence of the field which, in a uniform field, falls to zero. After experimental disturbances have been allowed for, all these influences have to be taken into account.

The more divergent the field the more does the simple diamagnetic effect assert itself, and any magne-crystallic action which would tend to make the specimen set a crystal axis or axes at some particular inclination to the direction of the field is overpowered.

THE CONTRAST BETWEEN PARAMAGNETISM AND DIAMAGNETISM

On the other hand, magne-crystallic action usually takes charge in a truly uniform field. For the sake of brevity and an easier explanation, it may be well to direct attention to a fact which was not fully appreciated by all the first experimenters, but was clearly set out by Sir William Thomson (Lord Kelvin) in 1885. A diamagnetic bar, apart from magne-crystallic action, tends to set itself *along* the lines of force in a *uniform* field, just as a paramagnetic bar. For we may imagine the bar to be made up gradually of a collection of cubes, placed successively one after the other in the magnetic field. The effect of the first cube is, as we should say in the language of Faraday, to spread out the lines of force on their way through the cube, and to crowd them together on either side of it. A second diamagnetic cube will, if free to move, go to that part of the field where the lines are least crowded; it will therefore avoid setting itself beside the first cube and prefer to place itself in front or behind it. A third will continue the same process, and in the end the cubes will form a bar pointing along the lines. In the case of a substance such as iron, there is a double converse. The lines are most dense just in front and just behind the first cube; and a second cube will place itself in one of those positions because a magnetic substance seeks the strongest part of the field. Again, therefore, the bar grows along the lines, as in this case we know from experience (see Fig. 3).

It is quite certain that no one has ever seen the first of these effects, because it must be so minute and difficult to separate from others. We may safely infer it, as Thomson pointed out, because our theories of the electromagnetic field have been abundantly justified by other means. The susceptibility of bismuth, by far the most diamagnetic substance, is

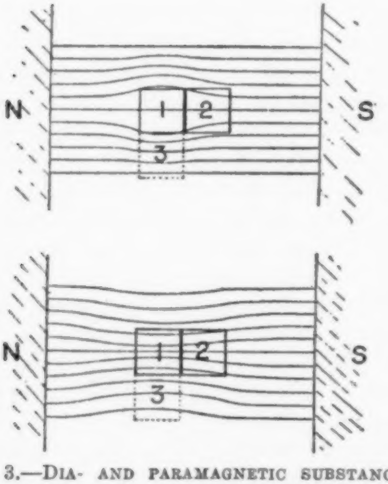


FIG. 3.—DIA- AND PARAMAGNETIC SUBSTANCES IN A MAGNETIC FIELD. IN BOTH CASES POSITION 2 IS PREFERRED TO POSITION 3.

only about 10^{-6}; in other words, the strength of the field on one side of a bismuth cube of 1 cm side would only be about a thousandth part of 1 per cent. greater than at the front or back of the cube. Near a pointed pole the strength of the field might easily vary by 50 per cent. in a centimeter. It is easy to see how feeble is the force tending to arrange the supposed cubes parallel to the lines of a uniform field as compared with the forces acting on a bar placed near the pole.

The analogous effect in electrostatics can, however, be realized. When two plates are immersed in oil and maintained at a large difference of potential, an elongated rod of glass hung from a fiber sets itself along the lines of force (to make sure that the effect is true the rod must be free from any conductivity due to its own substance or a water film). This corresponds to the setting of a magnetic body in a uniform field. When bubbles of air are allowed to rise through the oil they are[3] drawn out along the lines of force; which effect, since the inductivity of air is less than that of oil, represents the setting of a diamagnetic body along lines of magnetic force.

When a diamagnetic substance sets itself across the lines of a magnetic field, and no magne-crystallic action is at work, it is because the field is not really uniform. It is perhaps a little confusing when it is said, as is sometimes the case, that diamagnetic and paramagnetic substances are the antithesis of one another in that one kind sets itself across the field and the other along it. This is only true of a field which is non-uniform. Indeed, it may be said that the use of the word antithesis is incorrect in any case. There would be a true antithesis if one substance could be defined by its pointing along the lines of force in one direction while another pointed in exactly the opposite direction; there is no true antithesis between pointing along the lines and pointing across them. It seems to me, though I say it with diffidence, that this difficulty was stirring in Faraday's mind and was the true cause of the uneasiness of which he spoke in a quotation given above, and of his aversion to the description of diamagnetism and paramagnetism as being the antithesis of one another.

Faraday, as I have said, when this thorough examination of the facts had led him to frame a hypothesis which would link them together, based his interpretation on the existence of lines of force, and found himself able to place both his own results and those of others in their place within a self-contained system.

Kelvin placed this hypothesis in mathematical form, thus completing the treatment of the subject of magnetism by Poisson; the latter had left out of his consideration the consequence of magnetic susceptibility being different in different directions, not because he overlooked the possibility of such an effect, but because no case of its occurrence was known to him.

[3] My authority is Capt. Dunsheath of the Henley Telegraph Works Co.

THE HYPOTHESIS OF POLARITY

Faraday's views were not accepted, however, by other experimenters on the same subject, and in particular by Tyndall. The idea of polarity was not to be given up easily, and innumerable experiments were made to show that a "diamagnet" had poles like a magnet, but in the opposite sense. A bar of bismuth would develop north and south poles when, in similar circumstances, a bar of iron would develop south and north. Of course, when the facts are prepared for mathematical treatment, they can be expressed in this way. It is generally convenient and justifiable to represent a magnet by two poles because the form of the field at any reasonable distance is satisfactorily represented thereby, though in the immediate neighborhood the lines of a real magnet do not run like those of the theoretical bipole. Within the body of the magnet the lines run from the south pole to the north, continuing and completing their course outside so that every line is a closed circuit; but all lines near a bipole run from the north pole to the south pole. So also in the magnetic shell, which is in the mathematical treatment the exact analogue of the electrical condenser, the bulk of the lines run from one plate to the other across the narrow space between the two plates; comparatively few run from the outside of one plate, through surrounding space, to the back of the other. In the condenser, the internal field is the most important, the external being looked on as a correction. In the magnetic shell the reverse is the case; the outside field is that which is considered because it represents more and more closely, as the plates are brought closer together, the field due to a current circulating about the contour of the condenser. The theoretical charges on the plates have to be made larger and larger as the plates are brought together, so that the strength of the outside field may remain the same.

Now, if a piece of bismuth is placed along the lines of a magnetic field, the lines avoid the piece to some extent, though, as already explained, the avoidance is extremely small. If we take the bismuth away and replace it by a feeble bipole consisting of south-pole magnetism, of proper amount, where the lines come out of the bismuth, and a corresponding amount of north-pole magnetism where they go in, the whole arrangement being made in a vacuum, or permissibly, in the air, we get (Fig. 4) in

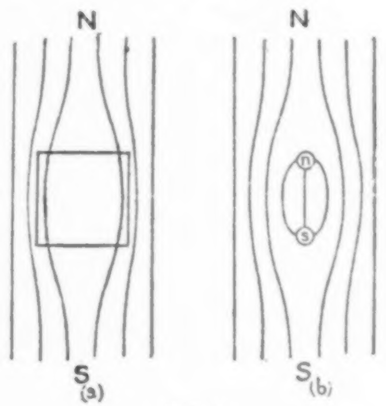


FIG. 4

this artificial way an external field resembling that which exists when the bismuth is in place. It can be said that polarity is developed in the bismuth in a sense opposite to that which is found in iron in the same circumstances. If the effect is to be represented, for the convenience of treatment, as due to the presence of a bipole, then the sense of the bipole in the case of bismuth is opposite to the sense in the case of iron. The old argument, therefore, was not between two hypotheses but between two languages in terms of which the facts were to be described. Such an antagonism, once believed in and debated, could be and was the cause of an immense variety of experiments devised to justify one side or the other. But Faraday felt that his way of putting the facts was more fruitful in suggestion of further experi-

ments, and more convenient as a foundation for theoretical development. He has been abundantly justified.

THE WORK OF TYNDALL AND KNOBLAUCH

We now come to the part which Tyndall played in a debate which was conducted on both sides in such an able and, it is pleasant to observe, in such a friendly way. In the first place, Tyndall and Knoblauch published in 1850 an account of experiments which they had made. They showed that Plücker's first views, those which included the repulsion of the optic axis of a uniaxial crystal by the poles of the magnet, were, as already stated, incorrect in many cases, and they substituted an amended set of rules in the following terms:

> If the arrangement of the component molecules of any crystal be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that chosen by the respective forces for the exhibition of their energy. If the mass be magnetic this line will set axial; if diamagnetic, equatorial.

The key-word is "proximity." This condensed statement of course requires explanation. Tyndall supplies it in full in his book on "Diamagnetism and Magne-crystalline Action." A very brief summary will be sufficient for our present purpose. In the first place, the observations made by him and his partner on the behavior of crystals in the magnetic field convinced them that the plane of cleavage determined in a number of cases the position which the suspended crystal would take. Magnesium sulphate, zinc sulphate, saltpetre, and topaz were diamagnetic substances, and their cleavage planes, the crystals being so suspended that these planes were vertical, always set themselves equatorially, *i.e.*, at right angles to the field. On the other hand, nickel sulphate, scapolite and beryl, which were magnetic crystals, in the same circumstances set their cleavage planes parallel to the field.

The connection between these results and the statement quoted above lies in this, that the molecules in a crystal were supposed to be in greater proximity along a cleavage plane than in any other direction. Let us take bismuth as an example; it is diamagnetic and sets its cleavage plane equatorially in accordance with Tyndall's rule. Its structure has now been determined by X-ray analysis, so that we can see what meaning can be attached to the claim for proximity in the plane of cleavage. The bismuth structure can be looked on, approximately, as a slightly distorted cube; one of the cube's diagonals has been a little stretched, while the other three have been left unchanged. The crystal is therefore uniaxial; the axis is the stretched diagonal. The principal cleavage plane is perpendicular to the axis. The spacing of the planes parallel to the cleavage is larger than that of any other set of planes in the crystal, and these consequently contain more molecules to the unit area than any other planes. Tyndall would have said that in those planes there was a maximum proximity between the molecules.

The X-ray analysis of other crystals often shows the cleavage plane to have the largest spacing and therefore the closest degree of packing. This means that the points of the crystal lattice are closest together in that plane, but it does not mean that the atoms or molecules are nearer together in that plane than in any other. Nothing can be said about that until the actual distribution of the atoms in the unit cell has been determined. It would be much safer to say that the existence of a cleavage plane implies a certain *looseness of packing* across the crystal planes which are parallel to the cleavage. This would imply a greater tightness in other directions, but not necessarily a greater proximity. It is only at first glance that the latter term seems to have a clear meaning. But we must let it stand in order to realize the argument

as it presented itself to the authors of the statement quoted.

It happens that in the case of bismuth we do actually find a closer bonding between the atoms in the cleavage plane than in any other; but this is peculiar to the structure of bismuth and has no relation to the supposed close proximity of *molecules* in the cleavage plane.

Now we come to the essential point of the argument. It is supposed that proximity offers magnetism or diamagnetism, whichever it may be, the opportunity to "exhibit its greatest energy." We are to remember that the hypothesis on which we are working expresses itself in terms of poles and that a magnet attracts a piece of iron by inducing poles in it, which poles then react with the poles of the magnet. When a piece of iron is allowed to attach itself to a magnet, the poles induced in it are much stronger than if the magnet and the iron are separated by a little distance. If a second piece of iron is brought near the first, every increase in its proximity increases the strength of the poles which are developed in this piece by the influence both of the original magnet and of the first piece of iron.

A simple experiment will serve as an illustration (Fig. 5): The nail hangs in the first case, and not in the second because the close proximity of the iron blocks increases the strength of the poles in all of them. In the second case, the nail will not hang, although the magnet is actually closer to it. The benefit of mutual "proximity" of the separate pieces of iron is obvious. An equally simple explanation can be given in terms of lines of force, but we are using the alternative language.

Chains of iron fragments form readily between magnet poles of opposite nature. A rod of iron "transmits the magnetic force," and generally acts more efficiently than a set of iron fragments which are not allowed to get into close proximity with each other. Tyndall sticks short lengths of iron wire through disc-shaped pieces of apple and shows that the disc sets itself at right angles to the field, the bits of wire therefore lying parallel thereto. In each bit are many molecules of iron in close proximity, and the fact is more effective in directing the apple than the existence of a number of bits scattered over the disc without being in "proximity" to each other.

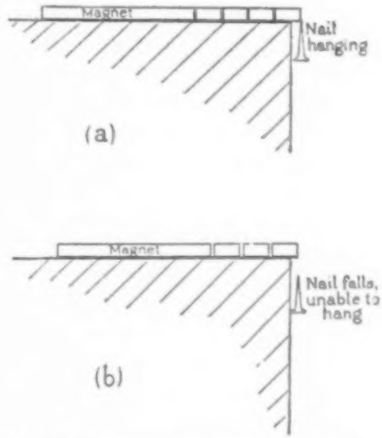


FIG. 5.—WHEN PIECES OF IRON ARE ALL IN CONTACT WITH ONE ANOTHER AND THE END ONE WITH THE MAGNET, THE NAIL CAN HANG AS SHOWN IN (*a*). BUT WHEN, AS IN (*b*), THE IRON BLOCKS ARE SOMEWHAT SEPARATED FROM ONE ANOTHER AND FROM THE MAGNET, THE NAIL FALLS.

DIAMAGNETISM AND "PROXIMITY"

It is now argued by Tyndall that if the magnetic influence of a magnet is extended by means of proximity, the diamagnetic influence must be extended in the same way. If the close proximity of iron fragments will help them to set with greater firmness in the direction joining opposite poles, then the closer proximity of bismuth fragments should cause them to set with greater firmness across them. In this way Tyndall interpreted the rule, which he believed he had established, that the cleavage planes of magnetic crystals tended to set axially, and those of diamagnetic crystals equatorially.

It is interesting to observe that Tyn-

dall was attempting to supply both a rule for the setting of crystals and an explanation of the rule in terms of structure. Faraday stopped short when he had supplied a picture of the distribution of his magnetic lines, or, as we should now say, a map of the distribution of energy in the magnetic field.

As I have already pointed out, there is no clear meaning to the term "greater proximity in the cleavage plane." Moreover, if conclusions were to be drawn from analogy with phenomena on a larger scale, they would run contrary to the intended argument; for, on that scale at least, a line of diamagnetic masses tends to set itself axially, not equatorially. A piece of bismuth makes an extremely minute alteration in the disposition of the lines of force, for which reason it is a very poor detector of the existence of the lines in comparison with iron; and the change, since it is so small, can indeed be detected by a piece of iron in the form of a magnet, but certainly not by another piece of bismuth, no matter how close they are together.

In the case of a uniaxial crystal, a principal cleavage must from symmetry considerations be related to the axis and, if it is unique, must be perpendicular thereto. When there is more than one cleavage, the cleavage planes must be symmetrically disposed about the axis as in the case of Iceland spar. It is not, therefore, surprising that, in the former case, the cleavage plane should place itself exactly, either equatorially or axially, and that in the latter case a plane perpendicular to the axis might be looked on as a resultant of cleavage planes and therefore set itself equatorially. But it does seem remarkable that, as Tyndall pointed out, the substitution of iron for calcium in Iceland spar, to form the isomorphous iron carbonate, should turn the structure round through 90° in the magnetic field; especially if we assign diamagnetism and paramagnetism to different causes. It may be there are other cases of the same change; and any rule of this kind must clearly be of importance.

The Effects of Pressure on Magnetic Susceptibility

We now come to another set of experiments, very interesting and important, which were used by Tyndall in the defense of the "polarity" position. The method of these experiments was suggested by an accident. When working in Berlin with a fine magnet placed at his disposal by Magnus, he was observing the action of the magnet on a bismuth cube which was so shaped that two opposite faces were perpendicular to the optic axis and parallel to cleavage planes. When the current was switched on, the magnet poles rushed together because the separate parts of the magnet had not been properly bolted down. The bismuth cube was crushed to some extent. Working conditions having been restored, it was found that the bismuth set itself at right angles to its former position. The line of pressure, which, of course, had been parallel to the field, was now perpendicular to it. Tyndall now argued that the particles of bismuth had been brought into greater proximity by the pressure and that the setting of this line of great proximity was in accordance with the rule given by himself and Knoblauch. So began an extended series of researches on the effects of pressure which are fully described in his book. As an example let us take the following:

> A quantity of bismuth was ground to dust in an agate mortar, gum-water was added, and the mass was kneaded to a stiff paste. This was placed between two glasses and pressed together; from the mass when dried two cubes were taken, the line of compression being perpendicular to two of the faces of each cube and parallel to the other four. Suspended by a silk fiber in the magnetic field, upon closing the circuit the line of compression turned strongly into the equatorial position. . . .

When carbonate of iron was used the line of pressure set axially.

Such an experiment is very striking,

whatever its explanation may be. Tyndall argued that he had by compression increased the proximity along the line of pressure, but it is difficult to see how this can be. If a number of particles of one kind are distributed with complete irregularity in a paste medium which is then subjected to pressure in one direction, the alteration in form of the paste block will not alter the law of distribution of the particles. In any case, as we have already seen, proximity does not produce any observable effects.

Lord Kelvin's Explanation of Tyndall's Results

It is surely natural to suggest that the particles acquired some orientation from the pressure, which might well happen if they possessed shapes which were related in some particular way to their structures. Thomson immediately pointed this out to Tyndall, who replied that if that were the case, the bismuth fragments being naturally in the form of flakes parallel to the cleavage plane, the line of pressure ought therefore to set itself axially, whereas it actually set equatorially. This was certainly a good reply. Perhaps the counter argument is that the crystal fragments have not actually been shown to set in this way. Miss Knaggs has made an X-ray measurement of the set of the fragments in one specimen of squeezed dough containing bismuth particles, and has found that the cleavage planes are not closely coplanar with the surface, as they must be if Tyndall's argument is to be good. Though this is a single example, it looks as if a way of escaping the difficulty was to be found.

As I have said, Tyndall's reply to Thomson was good, but, to use his own words, though it formed "a strong presumptive argument it was not yet convincing." He strengthened his case greatly by a further experiment. Comparing the repulsion exerted by a magnet on a natural crystal of bismuth with that exerted on a mass of compressed powder in dough, he found the latter greater than the former. He had cut the crystal into the form of a cube and placed it on one arm of a torsion balance so that the cleavage plane was perpendicular to the magnetic field, and the repulsive force as great as it could possibly be. The dough had been pressed into a cube of the same size and placed with its line of pressure at right angles to the field. Tyndall argued that there must be a direct effect of pressure, since it had done more than all that the natural phenomena could do.

Now it is clear that if the orientation of a bismuth crystal in a uniform magnetic field, *i.e.*, the magne-crystallic action, is due to the arrangement of the atoms and molecules in the crystal structure, the perfect crystal ought to show the effect more perfectly than the fragments distributed through the dough, however perfectly the latter may be arranged. But Miss Knaggs has made an X-ray photograph from the face of a natural "crystal." The specimen was chipped out of a mass of crystals left in a crucible, and must have resembled that which Tyndall used. The photograph showed at once that the specimen was a compound of more than one crystal, and that different orientations were present. Cleavage planes, and also others which in a single crystal would make large angles with the cleavage planes, were nearly parallel to the face under test. It is possible, therefore, that there was really more of the effective orientation in the pressed specimen than in the natural piece. A photograph of the single crystal made by Bridgman's method taken in the region of the cleavage plane gave a much cleaner picture.

A piece of bismuth can be looked on as an aggregate of crystals. There may be but one perfect crystal or there may be a number, small or large, of smaller crystals, each perfect. If proximity were increased by pressure, the change

in proximity would have to occur in respect to the mutual distances in either of the separate crystals, or of the atoms and molecules in the single crystal. The X-ray analysis shows that the latter alternative is impossible, because from many tests recently carried out in respect to metal structure, we learn that no permanent change in the crystalline lattice is occasioned by stress. The former alternative is also ruled out, because, as Faraday pointed out,[4] bismuth is actually of a lower density after compression than it was before; the pressure having of course been removed. Apparently the breaking up of the specimen increases the extent of the cavities.

Tyndall made many paste models of crystals, mixing powders of bismuth, carbonate of iron, or other active substances with flour and water, or gum. He pressed the mass by different amounts in different directions and then cut it to shape; in this way he imitated the magne-crystallic action in detail. At one time, in order to meet the objection that he was merely rearranging the small crystals in his paste and conglomerates, he took some white wax "concerning whose amorphism there can be but little doubt." The substance is diamagnetic. A little cylinder of the wax suspended in the magnetic field set with its axis equatorial. It was then placed between two stout pieces of glass and squeezed as thin as a sixpence; suspended from its edge, the plate thus formed set so that its length, which coincided with the axis of the previous cylinder, was axial and its shortest dimension equatorial. But we know now that wax is anything but amorphous; its crystalline structure has not only been observed but also accurately measured; and we know also that pressure arranges the orientation of the crystals.

Tyndall obtained the same result with a piece of bread, and we may repeat the experiment. A small piece of the crumb is squeezed between two glass plates, and the edges of the irregular mass are trimmed off, so as to leave a thin disc.

FIG. 6.—THE BLACK LINES SHOW DIFFERENT POSITIONS OF A THIN WAFER OF PRESSED BREAD HUNG BY A SINGLE FIBER. IN THE OUTER PARTS OF THE MAGNETIC FIELD IT SETS MORE OR LESS ALONG THE LINES, BUT AS IT IS BROUGHT UP TO THE MORE INTENSE PARTS, WHERE THERE IS GREAT DIVERGENCE OF THE LINES, IT TURNS SO AS TO SET ITSELF AT RIGHT ANGLES TO THE FIELD.

When this is suspended so that its plane is vertical, it sets equatorially if the poles are close together and the field is very divergent. It is therefore diamagnetic. But when the bread is moved from the space between the poles to a more uniform part of the field, the plane of the disc turns through a right angle and sets itself parallel to the lines of force. It is quaint to observe how the bread, as it is moved up to the poles, sets itself to pass neatly through the narrow gate and take up a parallel position on the further side. This is due to magne-crystallic action, so that the bread contains crystals, a fact easily verified by X-ray methods.

THE EFFECTS OF PRESSURE ON CRYSTALLINE CONDITION

The long series of interesting and ingenious experiments which Tyndall made to show that pressure produced proximity and proximity produced the equivalent of magne-crystallic action, must be held to have failed in their original purpose. But they will doubtless be put to a different use. They are related to a subject of immense importance in these days, namely, the effects of pressure and tension and mechanical treatment generally, upon the state of a material and upon its physical proper-

[4] His reference was to "Gmelin's Handbook of Inorganic Chemistry," vol. 4, p. 428.

ties. The consideration of such questions is fundamental to metallurgy and to other industries. The microscope has for many years been employed for the purpose, and the new methods of X-ray analysis are already being put into service. It may well repay us to consider Tyndall's experiments in a new light; and to examine the actual nature of those rearrangements which produced such remarkable changes in magnetic reactions. Tyndall himself discussed the effects of pressure in producing planes of possible cleavage, and was one of the pioneers in showing how such planes, occurring in the earth's crust, were not always to be interpreted as the result of sedimentary deposition, but rather of pressure, which might, if it were exerted more or less along the deposition planes, produce cleavages across the latter. He extended the principle to account for stratification in rolled materials, even in biscuits and pastry!

Faraday's use of lines of force did not, in reality, demand so much framing of hypothesis as Tyndall's polarity. It is to be observed that, as Faraday pointed out, they had no differences about facts, merely about methods of description, which methods, however, were of different value as suggesting development. To Faraday's conceptions have been added theories of magnetism and diamagnetism based on the existence of resistanceless molecular circuits as imagined by Ampère and Weber, or on revolving electrons as explained by Langevin. In the most recent times the quantum theories have again modified our ideas.

Modern Considerations

The crude hypothesis of the molecular circuit leads simply to a useful point of view of the difference between paramagnetism and diamagnetism, and the most modern discussions, though they differ greatly in appearance, leave that point of view almost untouched. If any of Faraday's lines of force thread a circuit which has no electrical resistance, that number can never be changed. If, therefore, a substance be brought into a magnetic field, the molecular circuits in the atoms of the substance act like obstructions to the lines; and the total obstruction, of which the negative magnetic susceptibility is a measure, is proportional to the sum of the areas of all these circuits, as projected on a plane perpendicular to the lines. It is of no consequence whatever whether there are already currents in those circuits; unless, indeed, the circuits are movable and can alter their set towards the imposed field. Thus the diamagnetism is unaffected by the existence of molecular magnetic fields; or by any changes in them, so long as the total of the projected areas of the circuits is unchanged.

This result does not hold if circuits approach each other so closely that they offer less obstruction to the lines than if they were more separated. Two resistanceless circuits running closely parallel to each other offer little more opposition to the passage of lines than either circuit alone. We should imagine that such changes in the relative position of circuits would only occur in strenuous circumstances such as, possibly, those of crystallization. It is known that diamagnetic susceptibility may vary very slightly: for example, Oxley has shown that crystallization sometimes brings about small but definite alterations. As has often been pointed out, this simple theory makes diamagnetism a property of all substances, which can be affected, even overwhelmed, when the circuits already contain currents, and therefore can be orientated afresh by the magnetic field.

Let me say in conclusion that although recently acquired knowledge of the structure of materials leads us to reconsider Tyndall's experimental results, we are still far from the full explanation of the connection between structure and magnetism, and of the influence of the latter upon physical properties.

THE DEATH PENALTY FROM A SCIENTIFIC POINT OF VIEW

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Apparently, the death penalty originated in this way: A long time ago, before the processes of common law had taken form, when a man was murdered, his relatives were given the privilege of revenge. They were allowed to administer their own justice; and they went out and got some one. But the doubt as to whether they always got the right one was too great. So the law removed this privilege of private killing—together with the burdens and risks that must have accompanied it if the other parties happened to be strong—and undertook to administer its own justice. And, little by little, arising thus from sources that pass back of the memory of man, the common law has built up a judicial process leading to the death penalty, which now entirely eliminates the idea of revenge, substituting for it the repression of crime, and which attempts to establish the maximum degree of certainty as to the guilt or culpability of the one who is accused. The methods for establishing this maximum degree of certainty, having likewise gone through several modifications, are now reduced to one basic principle, which is that the judgment of the case shall be free from reasonable doubt. So the death penalty depends upon the interpretation of *reasonable doubt*: What is the degree of confidence in a judgment that puts it beyond reasonable doubt?

This was the situation in the law, we may say, a hundred years ago. But more recently there has been arising a feeling, both within the law and elsewhere, that a degree of certainty that is beyond reasonable doubt is, perhaps, not so readily attainable as it appeared to be a hundred years ago. The standards of reasonable doubt have varied in the law from time to time; the judges have not always agreed about them. Moreover, the problem of the degree of certainty of a result has become, in the scientific field, one of the branches of learning, with standards now generally accepted that differ from those of the law, and with text-books discussing them. The degree of certainty of a result, or the confidence that may be placed in it, or its freedom from reasonable doubt, are merely different ways of expressing one and the same idea, the probability as to its accuracy, whether the result in question is a measurement, or an hypothesis or a principle or a judgment—they all exist in the scientific field quite as much as in the legal field. So it may be of interest to compare the legal and scientific methods and to observe how the law would be affected if the scientific methods and scientific conceptions were to be used in it. In a word, this effect would be that, by scientific interpretations, the legal methods do not reach a degree of certainty that is free from reasonable doubt.

The difficulties that exist in the legal methods for determining reasonable doubt may be seen from the citations that follow:

It is difficult to conceive what amount of conviction would leave the mind of a juror free from a reasonable doubt, if it be not one which is so settled and fixed as to control his actions in the more weighty and important matters relating to his own affairs. Out of the domain of the exact sciences and actual observations

there is no absolute certainty. The guilt of the accused, in the majority of criminal cases, must necessarily be deduced from a variety of circumstances leading to proof of the fact. Persons of speculative minds may in almost every such case suggest possibilities of the truth being different from that established by the most convincing proof. The jurors are not to be led away by speculative notions as to such possibilities. (120 U. S. 439.)

But the judges have felt, from time to time, in order to aid the juries, that they must give the term reasonable doubt a more definite meaning; as, for example, that a reasonable doubt is the antithesis of an abiding conviction—

. . . where there is an abiding conviction of the truth of the charge resting in the minds of the jury, there can not be at the same time, in the same mind, a reasonable doubt. (122 Ill. 8, 251.)

Such a definition was based originally upon a judge's view of the fundamental conceptions of law (5 Cush. Mass. 320). Thereafter, it was cited by other judges as the law; the legal substitute for reasonable doubt; that which determined it, and played its part in the destiny of many lives, sometimes for freedom and sometimes not. Then it fell into disuse because it no longer conformed to the conceptions of the judges. Now the tenor of the decisions seems to be that reasonable doubt needs no further definition (156 U. S. 185, 199; 218 U. S. 245). Other definitions followed a similar course; such as, *proof to a moral certainty*, *cause to hesitate and pause*.

These definitions have been severely criticized in the decisions themselves. "The words 'to a reasonable and moral certainty' add nothing to 'beyond a reasonable doubt' " (120 U. S. 430, 439). "Each has been used by eminent judges to explain the other" (118 Mass. 1, 24), "They are ill-advised efforts" (4 Wigmore, Evidence, Sec. 2497). And,

. . . it has been uniformly held that a reasonable doubt is one . . . such as, in the graver transactions of life, would cause a reasonable and prudent man to hesitate and pause. . . . The instruction [the abiding conviction definition] . . . in a case where the testimony is conflicting, or such as to leave the minds of the jury in doubt, [it] would be likely to prejudice the jury . . . we do not hesitate to condemn the instruction. (109 Ill. 636.)

Other definitions seem to depend upon these, involving the same difficulties.

Contrasted with these legal conceptions, the scientists have built up standards of certainty by which they determine the confidence that may be placed in a result, that is, its freedom from reasonable doubt; and these scientific standards now constitute a subject that is known as precision of measurements. Perhaps the alternate legal phrase, the reasonable certainty of a result, fits the scientific conceptions more directly; for the scientist determines what the degree of certainty of a result may be; and from this he establishes the conditions that will give the utmost degree of certainty attainable at any given time.

Scientific conceptions, in the first place, are never free from an underlying probability—it is even itself a feeling of certainty—that all its judgments will be modified at some time in the future. About all its judgments of the past have been modified, some of them many times. The atom, for instance, once generally accepted as the smallest division of matter, is now seen to be a veritable solar system, with a central "sun" and "planets," numbering perhaps to a hundred. The tube of every radio set establishes the certainty of this conception. And the facts shown there, and others like them, were before our eyes all the time that we were so certain the atom was indivisible; for who had not observed the blackening of an electric light bulb? Probably the most certain thing, in scientific experience, is that all judgments, however they may originate, will be modified in the future.

Next, it is the scientific conception that errors are inherent in everything;

and the accuracies of science are derived by analyzing those errors and assigning to them probable values. The sources of error are such as the apparatus, the observations, the observers, the processes or anything else that may tend to color or distort the result. The scientist analyzes his errors, not by comparing them among themselves, but by calculating the changes they produce in the result; thereby obtaining a figure that is known as the "probable error" of the result. For instance, if it is desired that a result shall be accurate to one per cent., then errors in contributing quantities that might change the result less than one tenth of one per cent., will not, on the whole, disturb the statement that the result is accurate to one per cent. So errors, if they can be kept within this limit, make no difference in the result, whatever their apparent size may be, large or small. Therefore, within such limits the scientist is able to make approximations wherein the differences between them and the true values will be indistinguishable in the result. Scientific accuracy is almost always limited by some one factor which, although determined with the utmost care, still affects the result far more than any of the other factors. But if, through improved facilities or the discovery of better methods, the accuracy of this difficult factor can be raised, then all the other factors are affected, because a higher standard of accuracy is now possible. What before may have been a reasonable approximation in some of the lesser factors—because indistinguishable in the result—is now no longer reasonable.

The difference between the apparent size of an error and the effect that it produces in the result may, perhaps, need some illustration. Let us consider the divergence of two lines as measured by the angle between them, which will bring out this comparison.

Suppose a man is lost in a wood, clouds completely obscuring the sky; and after walking some time he comes upon an object, say a toy compass, which he recognizes as his own, lost an hour before. He has been traveling in a circle. But the compass, however inaccurate, will get him out; for by means of it he will be able to follow the course he intends, a general easterly direction. It does not matter that the angle of his course with the line of the compass needle is purely a guess on his part; nor that the knife in his pocket may affect the needle; nor that the magnetic north differs from the true north. Such errors are of no consequence, because, if he can stick to any course, approximately, instead of traveling in a circle, he will come to the edge of the wood. So here we have an extreme case; an error obviously large in itself, which, when applied to the purpose at hand, becomes indistinguishable in the result.

The other extreme, a small error but which makes a large difference in the result, may be seen in artillery fire. In this, at certain positions, the slightest change in the angle of elevation of the gun, even a hair's breadth on the setting scale, will make a difference of many feet in the point where the shot strikes—it is the difference between a hit, and no hit.

The essential features for determining the confidence that may be placed in a result, its freedom from reasonable doubt, by the scientific method, are three: (1) that no stone small remain unturned where a source of error may lie concealed—a question of method; (2) that the effect of each error shall be judged, not by its apparent size, but by the amount of change it produces in the result, and this compared with the result taken as a whole—clearly a question of fact; and (3) that the standards of certainty of any given time shall not ex-

clude the probability of future modification. It is upon these points that the divergence takes place between the methods of science and those of law. If gauged by the scientific standards, it would seem that the law did not always take into consideration, in appraising error, sources where science finds error to exist; that it puts too much emphasis, at times, upon the apparent size of an error, as distinct from the effect that the error produces in the result; and, particularly as regards the death penalty, that it makes no allowance for the future modification of the error that it finds.

The scientific conception of error, then, if applied to the interpretations of law, would introduce there a doubt, or a degree of doubt, that perhaps had not been recognized before. It would look for error not only in the evidence, but in the processes of securing and bringing out the evidence as well; it would look for a source of error in the relative abilities of counsel who present the evidence; it would examine the "personal equation" of the various individuals who might be concerned in a judgment; and it would challenge the capacity of human beings to form judgments that may not be subject to future modification.

To ignore important elements of the "probable error" that the scientific point of view imposes upon judgments, whether of judges or juries or of others, and at the same time to imbue them with an infallibility and permanence commensurate with death, is totally incompatible with the conceptions and experiences of science. Under these conditions the term "freedom from reasonable doubt" becomes, to the scientific mind, illusive and meaningless. And even if it were possible to reconcile many divergent views of science and the law, there would still remain—while the death penalty remained—the very reasonable doubt as to what the future may bring forth.

So, it appears, that scientific conceptions and methods would give an interpretation of reasonable doubt which, if recognized in law, would render the death penalty no longer a rational adjudication.

But after all, the conflict between the death penalty and scientific reason does not rest entirely upon the scientific point of view; it would seem to lie directly in the facts themselves. Accepting it as a fact that the processes of common law must be rational ones; it is also a fact that a process intended to eliminate doubt, which leaves certain known sources of doubt—particularly the future—uninvestigated, is not a rational process, and, therefore, can not be the common law. And how can a structure supporting the death penalty rest upon that foundation?

ERWIN F. SMITH—A YOUNG MAN'S IMPRESSION

By DONALD CULROSS PEATTIE

"Well, go talk to Smith about it!" said my volatile chief, as he rolled down the top of his desk, crammed on his hat and strode off to a meeting with Vernon Kellogg or Walter Swingle or some other of his confreres.

I stood looking around the empty office, hung with pictures of Frank Meyer in Shantung and Rock in Burmah on the trail of chaulmoogra, and Popenoe on the Andes. I felt a bit blank and wondered if I had not been treated to a bit of irony. "Go talk to Smith about it!" Who, pray, was Smith?

I had a scheme, a wild plan that I am still not weaned from, a foolish notion that since the galls from which tannin is obtained are in a sense disease reactions of plants, it might be possible by growing certain vegetables in pure cultures to inoculate them with some disease, sufficiently virulent and yet sufficiently benign, to give us a continuous supply of the best tannic acid. I took the idea to David Fairchild, always open to fresh viewpoints. He listened as long as he could. Then "Go talk to Smith," said he.

So I went out in the corridor and asked up and down for Smith.

"Oh! Erwin F. Smith!" said every one.

That was something different. Yes, I had heard of him, had indeed read his book on the bacterial diseases of plants some years before in college, had heard his name reverently pronounced. Erwin F. Smith, pioneer of the bacteriology of the vegetable kingdom, the man who a generation ago was laughed at for predicting that it would be found that plants had more bacterial diseases than man, and lived to see his prediction more than fulfilled. Yes, I knew who that Smith was. And taking my hat gingerly in my hand, I almost tiptoed into his laboratory.

I looked some time around among the shining autoclaves, the racks of cotton-stoppered tubes, the simmering kettles of culture media, and the piles of books, before I discovered a short man, with handsome silver hair and beard, cheeks pink as a child's, seated, in immaculate laboratory clothes, quietly reading behind a big desk. I mention these trivial details because, as it happened many times as I subsequently entered his office, I never found him in any other posture or different in appearance. Always the milieu of bubbling vessels and sterilizers, always the little man in spotless attire, never flying around his laboratory, never appearing to labor, ever deep in a French or German book.

But however deep his attention, he never pulled himself out absent-mindedly, but on that first occasion as on all others he looked up quickly, and I found myself gazing into the most brilliantly blue eyes I ever saw. Their depth was unfathomable, and a light of humor sparkled there unfailingly. Upon seeing me he smiled as though I were an old acquaintance; the smile, I was about to say, was saint-like—more precisely, it was intensely and beautifully human.

I introduced myself briefly and started to blurt out my ideas. "I have a plan—"

"Good," he said. "Where were you born?"

I told him and tried to get on with my story.

He wrote down my birthplace in a little notebook, and then hazarded my age at twenty-four. I admitted it, but still I was not yet allowed to have my say. My name had to be spelled out carefully, and when he had it down in detail, he suddenly astonished me by asking how my mother's cousins were! After that fifteen minutes were consumed in talk about his old town of Lansing, Michigan, his relatives and mine there. The conversation worked around to my years at Harvard and the men under whom I had studied. That led to the Office of Foreign Seed and Plant Introduction and what I was doing there (all this noted down from time to time) and then at last I was permitted to try to "sell" my wild-cat scheme about tannin.

Again, I may justify the relation of these details by avowing that it appeared, later, to be his invariable practice to save his memory by putting down in a desk book the humans and their problems that came to him, just as a botanist saves a specimen of a plant. His desk book ought to make interesting reading; it must be a sort of herbarium of humans.

The most imaginative flights in my plan did not appear to startle this mild little man, this man who was daring to believe things that were utterly unorthodox in science (I refer to his open-mindedness towards the possible organic cause of animal cancers). He heard me out sympathetically, offered to help me, and sent me away loaded with books, reprints, manuscripts and articles bearing on that subject and on many in which he was interested more than I.

A few days later a letter came from a stranger concerning tannin. He mentioned Smith's name, and I realized that my correspondent too must have breezed into that laboratory, where Smith fumbled back in his notebook, discovered my name and whereabouts, and put us in touch.

When next I entered Smith's rooms I found, to my surprise, that I had to explain myself all over again. This was a bit chilling; but soon I realized that a mind so deep in philosophic ideas could not be burdened with remembering details like myself. I did not allow him, however, to write me down afresh in his notebook, and he laughed boyishly at the joke on himself when I found for him the entry concerning me, of several months before. I have forgotten what I came to talk of that day, but I know that his mind was just then full of Pasteur, whose biography he had just translated into English. Somehow he gave me an insight into the *esprit* of the French chemist who turned out to be a biologist that I have never forgotten. It fortified me against the superficial, slurring impression which the public has recently been given in that bit of able journalism, "The Microbe Hunters."

Some months later I attended a dinner of the Washington Botanical Society given in honor of Smith's birthday—his seventieth, I think. The dinner would have been dull as usual but for the revelation of a man I had now learned to know as truly great. Smith was so modest, so painfully modest, that he blushed almost continuously while on his feet before an audience of men younger and less eminent by far, for the most part, than he. He laughed nervously, stammered sometimes and was plainly glad to break away after the dinner.

Yet in the fashion of people who are simply modest, he talked with a quality of surprising frankness. His words were an *apologia pro vita sua*, charmingly candid in their delight at his ripe old age and the years of solid accom-

plishment, humorous in the fashion that is called tongue-in-cheek, unabashedly interspersed with quotations from the poets, his favorite lines quite evidently being those of Browning—

Grow old along with me,
The best is yet to be,
The last for which the first was made. . . .

He closed his talk with a sonnet of his own, offered after Browning, Tennyson, and I think Shakespeare, with no more apology than a smile. The sonnet did not need apology, however. Later I learned that he was the author of a volume of poems, and if none of them take rank as literature, they are all pervaded by the sweetness, sincerity and good taste of their author. It is inviting the scorn of the littler men in science to have artistic tastes or fanciful mental play. This scorn apparently did not touch Erwin F. Smith any more than he feared an unorthodox method in experiment.

Some one else will write his biography. I do not know when or where he was born nor very much of his personal life. I have not read all that he wrote, nor will I ever accomplish that. He died in Washington, April 6, 1927; that date is the only one I know in connection with the finest, kindest and broadest scientist I ever met. His pride in his age was only equalled by his love of youth and tolerance of it. Of that love and tolerance I have had very good proof. Far too busy to remember me for long, Smith was a man whom I, a beginner in science, could never forget. He was the sort of man who makes young men say, "If I could, I would choose to be like him, fifty years from now."

THE PROGRESS OF SCIENCE

EDITED BY DR. EDWIN E. SLOSSON

Director of Science Service

THE SOLAR ECLIPSE OF JUNE 29

PEOPLE in England and Norway now have the opportunity of seeing the same rare phenomenon that was afforded to residents of the northeastern United States in January, 1925. Early on the morning of June 29, the shadow of the moon will sweep across England, the North Sea and the Scandinavian Peninsula, and people in this narrow strip will see the sun's disc obscured, while around it will shine the magnificent corona. That is, they will see it if the weather is clear, for there is only one chance in three that it will not be cloudy in England at the time.

But despite the probably poor weather conditions in England, no recent astronomical event has attracted so much popular interest. The last chances that Britishers had to observe eclipses were on May 2, 1715, and May 22, 1724. Both of these were observed by the great astronomer Halley, who is known to us as the discoverer of the periodic character of Halley's comet. Evidently there was some fear that the people of England would be unduly alarmed over the eclipse, for in a public announcement about it Halley said: "The like Eclipse having not for many ages been seen in the Southern Parts of Great Britain, I thought it not improper to give the Publik an account thereof, that the sudden darkness, wherein the Stars will be visible about the Sun, may give no surprize to the People, who would, if unadvertized, be apt to look upon it as ominous, and to interpret it as portending evill to our Sovereign Lord King George and his Government, which God preserve."

That eclipse of 1715 was visible from London and fortunate clear weather enabled Halley to observe it. Evidently he was very much impressed with the strangeness of the occasion, for he wrote afterwards of the "chill and damp with which the darkness of the Eclipse was attended, of which most Spectators were sensible and equally Judges," and the "Concern that appear'd in all Sorts of Animals, Birds, Beasts and Fishes upon the Extinction of the Sun, since ourselves could not behold it without some sense of Horror."

The eclipse of 1724 was also observed by Halley, but since then England has not been favored with an eclipse until this year. If clouds prevent observations this month, British astronomers will have a long wait ahead of them for the next eclipse, because it occurs on August 11, 1999. Then it will just touch the southwestern tip of Cornwall.

Norway seems about the best location, and that is where many of the astronomers who want to observe it are locating their instruments. Of course the English astronomers are making every preparation to watch it, in the hope that it will be clear, but Professor Samuel A. Mitchell, of the University of Virginia, who will head the only American expedition, has gone to Fagernes, in Norway. With him is Dr. Harlan T. Stetson, of Harvard University.

ERWIN F. SMITH

OF THE UNITED STATES DEPARTMENT OF AGRICULTURE, IN WHOSE DEATH AMERICA HAS LOST ITS MOST DISTINGUISHED PLANT PATHOLOGIST

SCIENTIFIC AIDS TO AVIATION

WEATHER maps in eight layers, showing what conditions the aviator may expect when he flys both low and high are now being made regularly as a part of the official government weather report. Ground weather observations alone have been found to be inadequate for flying use, and the high altitude observations have also been found useful in making the regular weather forecasts. In fact, the weather seems to brew at the higher levels and the observations, obtained by small balloons and kites, often give information unobtainable at the level of the earth's surface.

The data used in the upper air weather maps come to the Weather Bureau from thirty-five balloon stations throughout the country. There will be eight more after July 1. The Army has eighteen, while the Navy has ten balloon stations and one airplane observer. The Weather Bureau also receives air aloft reports of temperature and humidity from five stations where kites are sent up with meteorological instruments. As kites are dependent upon wind the Weather Bureau is about to try out a small captive balloon, just large enough to carry the necessary instruments, at Due West, S. C., for use when the air is "light." Helicopters will probably replace balloons and kites in the future.

Bumpy air, which makes rough riding for flyers, is clearly shown by these kite reports. One recent report from Grosbeck, Texas, showed three strata of air in an altitude of 1,800 meters. The complete aerological observation map gives wind direction, velocity, kind and direction of clouds, and visibility as at surface, 250 meters above, 500, 1,000, 1,500, 2,000, 3,000 and 4,000 meters above. The balloons sometimes achieve much higher altitudes. One in Washington climbed 22,000 meters. The origin of the wind, whether polar or tropical, is also known to the Weather Bureau and aids in weather forecasting.

As soon as Congress makes the necessary appropriation the U. S. Weather Bureau intends to take over the broadcasting of weather conditions now carried on from the bureau by the Navy through the Arlington Radio Station to all parts of the world.

"Flight surgeons" is the term used to designate a new class of specialists brought into being by the rapid advances of aeronautics since the world war. It has been tragically demonstrated that not everyone who wants to can fly and it is the job of these specially trained exponents of aviation medicine to spot such individuals before they get into the air, according to Major L. H. Bauer, of the U. S. Army Medical Corps. "An immense amount of work has been done, both from the experimental and practical sides, in this country and abroad," Major Bauer explained. "As a result of this work the percentage of aviation accidents due to physical causes has decreased in a surprising manner."

The outstanding essentials in the selection of a flyer, he explained, are good eyes and good nervous stability, quick reaction and coordination. By good eyes are meant not only normal vision and absence of color blindness but literally the ability to "see out of the corner of his eye," for in flying it is quite often necessary to know what is happening at one side when it is imperative to keep "eyes front" to see what is coming straight ahead.

Quick reaction time is essential because the flyer frequently has to meet a situation where almost automatic reaction is needed. The ear in spite of its connection with the sense of balance, is not now considered of great importance

MEDALISTS OF THE FRANKLIN INSTITUTE

THE MEDAL MEETING OF THE FRANKLIN INSTITUTE, PHILADELPHIA, WAS HELD ON MAY 18, WHEN PRESENTATION OF MEDALS WAS MADE TO SEVERAL LEADING AMERICAN MEN OF SCIENCE. THE PHOTOGRAPH SHOWS, FROM LEFT TO RIGHT, PROFESSOR EDWARD L. NICHOLS, OF CORNELL UNIVERSITY; DR. W. D. COOLIDGE, OF THE GENERAL ELECTRIC COMPANY; PROFESSOR HARLOW SHAPLEY, OF HARVARD UNIVERSITY, WHO RECEIVED THE MEDAL

in determining the physical status of an aviator.

After flyers are selected they are classified with respect to their ability to attain altitude, into two groups, fainters and non-fainters. The decreasing supply of oxygen in the far upper reaches produces physiological changes in the body to which different types react differently. Symptoms develop insidiously and the aviator may reach the fainting stage without realizing that he is not in a state of perfect well-being. Consequently a test has been worked out whereby flyers are grouped according to their altitude reaction, into classes A, unrestricted, B, restricted to 15,000 feet, and C, restricted to 8,000 feet. The limit of consciousness without oxygen is about 25,000 feet and with oxygen artificially supplied the limit of altitude is between 40,000 and 45,000 feet.

As ships put into good harbors, air traffic will surely be drawn to cities with well-designed airports, aeronautic experts believe. When aviators reach Atlanta they find on the top of one of the tallest buildings great letters spelling "Atlanta" and an arrow pointing the way to the municipal landing field.

The United States is following Germany, France and England in acquiring and improving municipally owned landing fields. For the purpose of perfecting a working organization for immediate development of American air fields and airways a National Conference for the Development of Commercial Aviation is being held at St. Joseph, Missouri. Porter H. Adams, of Washington, D. C., president of the National Aeronautic Association, is chairman of the conference.

As aeronautics expands commercially every city will need an airport. The requirements are that it shall be centrally located, easily approachable from all directions and by all methods of ordinary transportation. It should be large enough to be adequate for the increased needs of the future.

The development of emergency landing fields approximately every twenty-five miles along airways with boundaries indicated by lights, either acetylene or electric and with an incandescent searchlight type of light revolving in a horizontal plane has been the plan of the Post Office Department for several years. Floodlighted buildings and obstacles around the fields give a daytime perspective. It is believed that speed practically equal to that made during the daylight hours may be maintained at night over properly lighted and prepared airways and will make possible rapid transportation of mail, express and passengers for every section of the country.

THE EARTH INDUCTOR COMPASS

LINDBERGH attributed his success in keeping on his outlined course in his transatlantic flight to the earth inductor compass with which his monoplane was equipped. Regarding this instrument he wrote: "Laymen have made a great deal of the fact that I sailed without a navigator and without the ordinary stock of navigation instruments, but my real director was my earth inductor compass. I also had a magnetic compass; but it was the inductor compass which guided me so faithfully that I hit the Irish coast only three miles from the theoretic point that I might have hit if I had had a navigator. The inductor compass was so accurate that I really needed no other guide." In his flight to Germany Chamberlin was less fortunate in the use of the earth inductor compass and relied primarily on a simple magnetic compass. He deviated considerably from the course that he had laid out.

The working of an ordinary compass depends solely upon the interaction of

two magnetic fields, while the principle of the earth inductor compass is based upon that of electromagnetic induction. In 1831 Michael Faraday discovered that when an electric conductor is moved across a magnetic field a current is generated in that conductor. In the case of this compass the electric conductor consists of a rectangular armature which is driven by a windmill; and the magnetic lines of force that are "cut" are those of the earth field. The electromotive force generated by an armature depends upon the position of the brushes on the commutator in relation to the direction of the magnetic field. In general if the line joining the two brushes is perpendicular to the magnetic lines of force, the maximum potential is obtained; when they are parallel the potential is zero. The working of the compass hinges upon this fact. The potential depends upon the angular relation between the brushes and the direction of the earth's field. That is, the output of the generator is a function of the angle between the position of the brushes and magnetic North. The "indicator" or "compass hand" is simply the needle of a sensitive galvanometer, the position of which indicates the relative amount of electromotive force generated. A device mounted on the instrument board termed the "controller" is connected to the brushes by a shaft in a casing. Rotation of the controller causes a corresponding rotation of the brushes of the generator. Dials upon the face of the controller show the angle through which the brushes have been oriented in relation to the airplane.

The usual method of steering by this compass is to set the desired heading on the controller and then to steer to keep the indicator on zero. An unknown direction can be determined by rotating the controller dial until the indicator reads zero, when the course will correspond to the point at which the controller dial stopped. This dial has 30 divisions, each one corresponding to 10 degrees on the compass, making in all 360 degrees. Operating at normal speed, the generator output will be sufficient to cause a deflection of the tip of the hand of the indicator of not less than 0.4 mm. for a 1° departure from course, and a deflection of not less than 10 mm. for a 30° departure from course.

There are two outstanding advantages of the earth inductor compass over the ordinary magnetic compass. First: the generator is mounted in the rear of the fuselage—so far distant from the motor that any magnetic fields created by it can have no effect on the deflection of the galvanometer needle. Second: the needle automatically always tends to return to the zero point through the action of a delicate spring. This compass is thus relatively unsusceptible to vibrations and other disturbances which tend to prevent the proper functioning of the ordinary magnetic compass.

Dr. Paul R. Heyl and Dr. Briggs, of the Bureau of Standards, constructed the first working model in 1920. After extensive tests by the Army Air Corps, further improvements were made and the instrument has withstood all the critical tests to which it has been subjected. It was then turned over to instrument makers for commercial production.

LANGLEY'S AERODROME

THE first announcement of Langley's experiments with his aerodrome was made thirty-one years ago in the following communications:

THE EDITOR OF SCIENCE—*Dear Sir:* After having published some investigations in aerodynamics ("Experiments in Aerodynamics" and "The Internal Work of the Wind"), I have made further experiments on the practical application of these conclusions, in the construction of an aerodrome or flying machine, upon a scale sufficient to admit of the employment of a steam engine of between one- and two-horse-

AUGUSTIN JEAN FRESNEL

THE FRENCH PHYSICIST, DISTINGUISHED FOR HIS WORK ON OPTICS, THE CENTENNARY OF WHOSE DEATH OCCURS ON JULY VD

power. I have never given any account of these experiments, as I have wished first to attain such a complete control of the flight as would insure its being automatically directed in a horizontal course, in any desired azimuth; but in view of the demands upon my time, which render it uncertain how far I can continue my personal attention to the completion of this object, I have yielded to the request of my valued friend, Dr. Graham Bell, to authorize the publication of a general statement of the results thus far obtained.

Let me add, in explanation, that the scale of the construction did not admit of any apparatus for condensing the steam or economizing the water, which, therefore, could only be carried in sufficient quantity for a very short flight. This difficulty is peculiar to the scale on which the experiment is conducted, and does not present itself in a larger construction.

Professor Bell has shown me his letter, which follows.

Very respectfully yours,
S. P. LANGLEY

Washington, D. C., May 12, 1896.

THE EDITOR OF SCIENCE—*Dear Sir:* Last Wednesday, May 6th, I witnessed a very remarkable experiment with Prof. Langley's aerodrome on the Potomac River; indeed, it seemed to me that the experiment was of such historical importance that it should be made public.

I am not at liberty to give an account of all the details, but the main facts I have Professor Langley's consent for giving you, and they are as follows:

The aerodrome or "flying machine" in question, was of steel, driven by a steam engine. It resembled an enormous bird, soaring in the air with extreme regularity in large curves, sweeping steadily upward in a spiral path, the spirals with a diameter of perhaps 100 yards, until it reached a height of about 100 feet in the air at the end of a course of about half a mile, when the steam gave out, the propellers which had moved it stopped, and then, to my further surprise the whole, instead of tumbling down, settled as slowly and gracefully as it is possible for any bird to do, touched the water without any damage, and was immediately picked out and ready to be tried again.

A second trial was like the first, except that the machine went in a different direction, moving in one continuous gentle ascent as it swung around in circles, like a great soaring bird. At one time it seemed to be in danger as its course carried it over a neighboring wooded promontory, but apprehension was immediately allayed as it passed 25 or 30 feet above the tops of the highest trees there, and ascending still further its steam finally gave out again, and it settled into the waters of the river, not quite a quarter of a mile from the point at which it arose.

No one could have witnessed these experiments without being convinced that the practicability of mechanical flight had been demonstrated.

Yours very truly,
ALEXANDER GRAHAM BELL

1331 Connecticut Avenue,
Washington, D. C., May 12, 1896

THE GUGGENHEIM SCHOOL OF AERONAUTICS OF NEW YORK UNIVERSITY

GROUND was broken on October 22, 1925, at University Heights in New York City, for the Guggenheim School of Aeronautics of New York University. The building which had long been a dream in the minds of the university authorities came about through the generosity of Daniel Guggenheim who, in making his offer and presenting his check for $500,000 to Chancellor Elmer Ellsworth Brown, was actuated by a desire "more quickly to realize for humanity the ultimate possibilities of aerial navigation" and "to give America the place in the air to which her inventive genius entitles her."

The new building of the Daniel Guggenheim School of Aeronautics, the construction of which began in the fall of 1925, was formally opened on June 4 of this year. A large gathering of prominent men identified with the aviation industry and the government inspected the building from cellar to roof. Mr. Daniel Guggenheim spoke to the visitors and the Honorable William P. MacCracken, assistant secretary of commerce, carried the good wishes of the government to the officials of the school and university.

In 1921 the growing importance of aviation led to an investigation of its possibilities by the department of me-

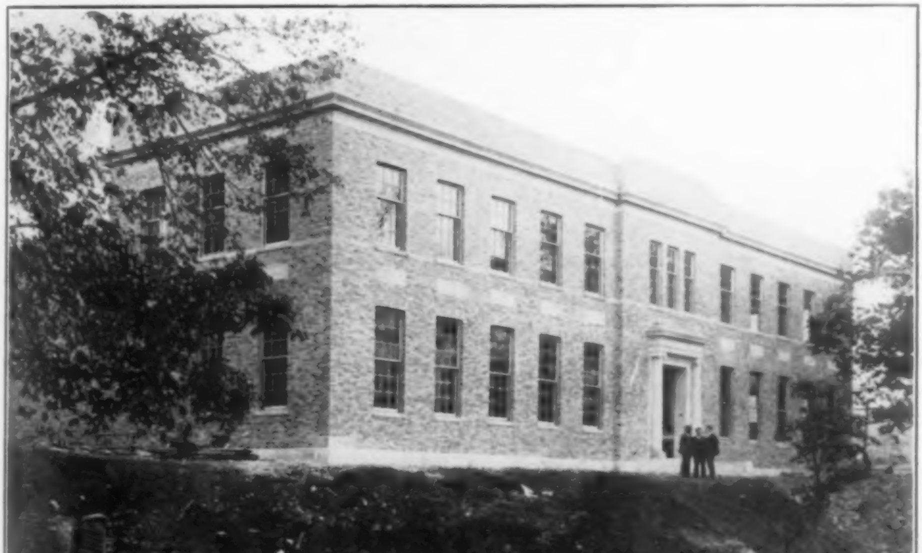
chanical engineering of the university with the result that a course of lectures in aeronautics was given to senior students during the academic year of 1921–22. The success of these lectures led to the incorporation of courses in aeronautical engineering as a senior option with an introductory course in aerodynamics and airplane design in the junior year. The first class was graduated in 1924.

In order to place aeronautical work of the university on a permanent footing, an advisory committee formulated plans for a permanent endowment which the generosity and far-sightedness of Mr. Daniel Guggenheim quickly made possible.

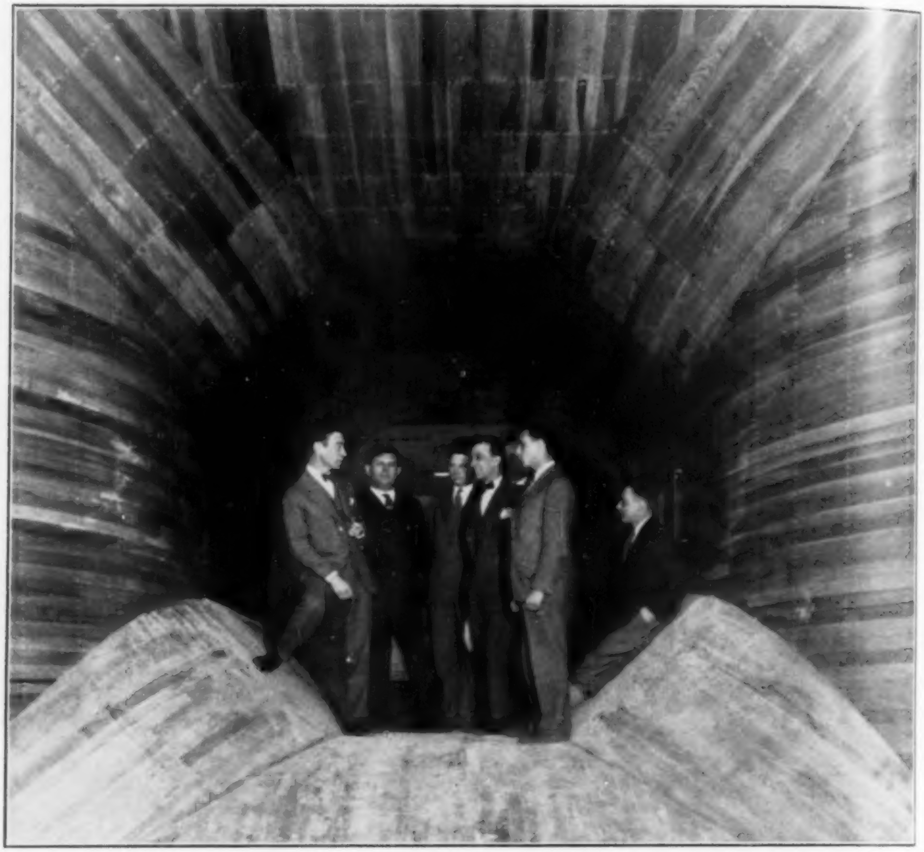
The new building which is located on the south side of the 50-acre campus of the university at University Heights, the Bronx, in New York City, contains one of the largest wind tunnels in the world wherein tests will be held involving models of airplanes and other aerial dynamic structures.

The air will be driven through the wind tunnel, which is 110 feet long and 55 feet wide, by an eight-bladed aluminum propeller, 14 feet in diameter and driven by a 300 horse-power motor which will create a gale force of 100 miles an hour. This will permit of the most exact results which will accurately forecast flying conditions affecting full-sized planes.

The building, which is of light brick trimmed with stone, is similar in architecture to the other buildings on the campus. In addition to the wind tunnel the building contains a structural laboratory where flying instruments will be tested before they are used in checking aeroplane action in flight; a shop where models of planes, accurate to one two-hundredth part of an inch, will be built; a large drafting office; a laboratory for testing aeroplane engines; an aeronautical library; an aircraft museum; class rooms and offices. The Air Unit of the University Reserve Officers



THE GUGGENHEIM SCHOOL OF AERONAUTICS OF NEW YORK UNIVERSITY



WIND TUNNEL
OF THE GUGGENHEIM SCHOOL OF AERONAUTICS

Training Corps also has their headquarters in the building.

Charles H. Snow is the dean of the College of Engineering and Professor Alexander Klemin, professor of aeronautical engineering, is in direct charge of the work in the new School of Aeronautics.